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Current Anthropology

A WORLD JOURNAL OF THE SCIENCES OF MAN

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CURRENT ANTHROPOLOGY (現代人類学) は形質人類学、文化人類学、社会人類学、言語学、考古学、先史学を含む人類学的諸科学にたずさわる学者の世界的共同体を対象とする雑誌で、年6回英語で刊行される。CURRENT ANTHROPOLOGY の会員はこの雑誌を通して、お互に知識見解を交換する学者の協力団体を構成し、全世界にわたる完全で自由な知識の交換に努力するものである。CURRENT ANTHROPOLOGY の機関会員となるべき手続をうけた学術機関も会員と同様の義務をもつ。個人および機関としての会員は雑誌 CURRENT ANTHROPOLOGY の配布をうけ、ただ名義的な会費 (一般購読料の 20%) を払う。一般購読料よりずっと低い額を払う理由は、後述する様な積極的な参加という義務をもつからである。上記の人類学のいずれかを既に専攻している学生および隣接科学を専攻する学者は、会員の推薦により特別料金購読者 (購読料は一般の 40%) となることができる。一般購読者には資格を問わず誰でもなることができる (裏表紙の表参照)。

歴史

ウェンナー・グレン人類学研究財団 (Wenner-Gren Foundation for Anthropological Research) は、1941 年の創立以来、人間に関する諸科学の間の広い世界的な学術交流の意義を認め、1952 年には国際人類学シンポジウムをスポンサーし、その結果として *Anthropology Today. Appraisal of Anthropology Today* および *International Directory of Anthropological Institutions* の出版をみるにいたった。つづいて 1955 年には、試験的な *Year Book of Anthropology* が出版された。その一部分は 1956 年に *Current Anthropology* の題名で出版されたが、この題名から現在の誌名がとられたのである。

この目標に向かって、ウェンナー・グレン財団は 1957 年 CURRENT ANTHROPOLOGY を設立した。そして CURRENT ANTHROPOLOGY をどのような形の出版物にすべきかという問題について、全世界の学者と会議および通信による討議が行われたが、オーストリアの Burg Wartenstein にウェンナー・グレン財団のヨーロッパ本部を開設した会合において、CURRENT ANTHROPOLOGY に関する次の大綱がきめられたのである。

1. 雑誌は人類学的諸科学に関するすべての問題を包括し、広い範囲にわたる多種多様の相関連する見解や資料を一ヶ所に集め、国際的な相互連絡を促進すること

2. 常に問題を全体的・総合的な背景と関連において扱うこと (holistic).

3. 人類学者が現在行なっている研究活動について、情報を交換しうる手段を提供すること。

以上の原則は、さらに翌年、世界各地で開いた会合に参加した人類学者の協力をえて現在の形に発展したのであった。

会員の資格

CURRENT ANTHROPOLOGY は国籍や政治的立場をとわず、全世界の学者、学術機関をつなぐものである。会員はほんの名義的な会費を支払うかわりに、新会員を推薦し、論文、ニュース、その他か他の会員が興味をもつ資料を提供し、また編集者からの論文の批評の依頼にこたえることによって参加協力することが期待される。

内容

CURRENT ANTHROPOLOGY の内容は2部門に分けられる。すなわち評論およびニュース・参考資料。CURRENT ANTHROPOLOGY はもとより他の雑誌にのる様な論文をも掲載するであろうが、本誌は地域的にも理論的にも、非常に広範囲なものを取扱うわけであるから、他の雑誌の長々しいくりかえしになる様なことはまずあるまい。本誌は人類学的諸科学にたずさわるすべての学者のための手形交換所としての斬新な機能をもつ。

評論 Review Articles

ここに評論というのは、人類諸科学に関連したあらゆる題目についての知識の大観であるが、現在にいたるまでに充分知られたものの総合というよりも、人類学の発展に伴う新しい資料、新しい評価であることが望ましい。しかしこの様な新研究は、必ず既知のもの脈絡の中に置かれねばならぬ。評論は、文献、資料、研究、方法などの論評であることもあろう。またその範囲は、時代や地域やその他の規準によって限定されることもあろう。ただ、つねにその基線となったものを明示するか、またはその基線として役立つ過去における発展の簡潔な要約を含んでいなければならない。その題目は範囲の広さにおいては野心的で、取り扱うところは包括的に、よく具体的な例証に裏付けられ、説明的であると同時に発展性をもったものであるべきだ。充実した参考文献目録は必須である。筆者は他の諸分野の専門家に呼びかける一つの分野の専門家であることを常に念頭におき、記述はできるだけ簡明にわかりやすくし、特に概念を表わす術語について充分な注意を払われたい。

論文の長短は、それぞれの論文の取扱う素材によっておのずから決定されるが、内容にどれだけのものを選ぶかという規準は筆者の良識にまたねばならぬ。写真、図版などは必要な限りのせることができる。

論文が雑誌にのるまでは、普通、以下の様な操作をへる。まず、論文が投稿されると、編集者は一応これを受諾し、いくつかのコピーをつくり、会員の中、その問題の専門家であるか、またはあきらかにその問題に興味をもっていると思われる何人かの人びとに、批評を依頼するためコピーを送る。これはわれわれの使命とする学者同士の建設的な交流の第一歩である。たんに投稿論文の批評を求めるとだけでなく、その批評自体を“原稿”として取扱う。これを雑誌にのせるかどうかは編集者の判断による。編集部には、すでに会員によって書き入れられた専門の、または興味をもつ、問題や地域を記したカードがファイルされてあるので、編集者はこのカードと論文の筆者の希望とにもとづいて、批評を依頼する人びとを選



Our Readers Write

One great contribution of *CURRENT ANTHROPOLOGY* could be a more concentrated fact-yielding style for anthropological communications that would assure readership for an article, while pointing up what the author had to say.

We have perpetuated wordy meandering, over-quoting, and not quite coming to any point. Lacking a rigorous "findings" section, perfectly good data have never seen print and the adequacy of a contributor's investigation has to be inferred from context.

At the least, an abstract like those in the Technical Communications section of *Science* would help. Minimal data—such as the population sampled (N), number of subjects (Ss) actually interviewed, and adequacy of response from informants (I) would do much to accomplish this aim. "For 132 nuclear families with a total of 463 Ss, out of a population of 8,712, Fa-Da influences were greater than Mo-Son, with 90% agreement among 23 informants" may not be great writing. Such summaries, however, would benefit the student and sharpen the contributor's wits as well. If one has something to say, it can usually be summarized.

To summarize, then, shorter, terser articles, better buttressed by data, and summarized in minimal words may well become current anthropology. Summary of this letter—fewer words, more data.

STANLEY M. GARN

As an archaeologist and Pacific specialist, I find my special bibliographic needs and reports of current work satisfied by *COWA* and the Far Eastern Prehistory Association. Two essential functions *CURRENT ANTHROPOLOGY* can perform, however, which it is difficult to see can be provided elsewhere:

(1) Extended reviews of current and perennial issues of theory and methodology within the various anthropological disciplines and between them. In the one category might fall, for example, problems of quantification of data in archaeological research and their bearing on excavation methods; in the other, the possibility of ethnog-

raphers' establishing correlations between cultural phenomena that might be useful to archaeologists contending with prehistoric societies for whom the bulk of the evidence is lost.

(2) Previews of and reports on anthropological conferences, since the publication of the proceedings of such conferences is often a very protracted affair.

In this connection, the anthropological news stories are an excellent feature and perhaps could be extended. In subjects like physical anthropology and archaeology, important discoveries have real headline value and it is very important for teachers to have quickly a more authoritative report than is provided by newspapers, which ordinarily are the only public and general source of information for a considerable time after the discoveries are made.

JACK GOLSON

So far as African archaeology is concerned, your publication should be of great value. I hope preference will be given to brief articles recording newly discovered evidence, particularly with reference to our Iron Age, which has only been scratched. It is evidence we require, not theories, at this stage.

KEITH RADCLIFFE-ROBINSON

Each number of *CURRENT ANTHROPOLOGY* should be edited in two or three self-contained parts: (1) physical anthropology (anthropogenesis, human biology, typology); (2) ethnology, ethnogenesis, archeology; (3) sociology and history. There should be more articles on human biology and the mechanisms of evolution; and the list of Associates should be arranged according to the different problems with which Associates are concerned.

NAPOLEON WOLANSKI

I would strongly support W. W. Howell's suggestion (March, p. 194) of a regular column on new discoveries and dates. In México, all our knowledge of the Kwangtung skull is derived from newspapers; and the only other serious report on *Zinjanthropus* appeared in the *Illustrated London News*.

Perhaps someone would have the kindness to take charge of such a column of information. SANTIAGO GENOVÉS T.

Brief notes covering field activities throughout the world in the past few years and also in the future would be useful. These could include name, special subject, area, and time.

KLAUS FERDINAND

I hope *CURRENT ANTHROPOLOGY* will allocate a section for terminology, and invite representatives of all the different schools of cultural anthropology to define their usages. CA may afterward issue a supplement—a sort of dictionary of anthropology, which is badly needed to eliminate controversies and misunderstandings. Special reference should be given to the aims and purposes of Applied Anthropology.

MOHAMED RIAD

I was pleased with Douglas Haring's suggestion of "less academic and theoretical discussion . . . and a revival of emphasis on protracted field research that eventuates in insight . . ." (March, p. 81). I should like to emphasize this point of view: Field research in the future will require better preparation, especially a fair knowledge of the relevant *lingua franca*. The period when anthropologists received information through translators is now past.

As for suggestions that *CURRENT ANTHROPOLOGY* should publish in French as well as in English, I must say that an equally good argument could be offered for the use of German as well. But it is only fair to support the Editor's reasons for using English as the sole means of CA communication, since the introduction of a second language would only stimulate arguments for using a third one.

HERRMANN JUNGRAITHMAYR

The problem of the culture of the Nagas requires urgent working-over. In my opinion, this can only come about through close communal work between "Tibetologists" and specialists in the culture regions involved. A significant area for study is also the prehistory of Central Asia, especially of Tibet. I should like to see an issue of *CURRENT ANTHROPOLOGY* devoted to ethnological problems of Asia, and specifically, of Central Asia.

SIEGBERT HUMMEL

It seems to me that *CURRENT ANTHROPOLOGY* could adopt the system of *Anthropos* (St. Gabriel's College, Mödling, Oesterreich) or that of *Africa* (International African Institute, London, England), leaving the choice of language to the author of each article (of course, one should not use Albanian, for

example, but one of three or four languages of world-wide importance) and, if necessary, follow each article with a résumé in English and French. One must not lose sight of the fact that the preparation of a scientific article in a foreign language requires more effort and time than is always at the disposal of the author. If it is absolutely impossible to adopt this solution, and the journal must appear in English only, then it seems to me that the editorial staff could be responsible for the necessary translations.

RENÉ PHILIPPE

Sub-fields of Anthropology Reconsidered

The cover-symbol of CURRENT ANTHROPOLOGY indicates the range of anthropological sciences. Suggestions for changes in the symbol were reported in "Our Readers Write" for May (p. 260). Concluding that Associates wanted the symbol modified, the Editor suggested that, "Because there seems to be no limit to possible additions of sub-discipline names, the best solution may be to drop such names . . . The best compromise seems to be to generalize the traditional sub-disciplines; and my own tentative suggestion is to substitute for the names now on the symbol the terms "Cultural, Social, Historical, Biological Anthropology." Associates were then asked whether they preferred (1) the original list; (2) the substitute; or (3) still a different alternative of their own choice.

By the time this issue went to press, 221 replies had been received. 101 Associates favored the original list, while 51 preferred the substitute, and 56 were satisfied with neither; 13 had no opinion. Thus, of the 208 Associates who took a position on this question, 48.5% wish to retain the symbol in its present form; 24.5% wish it replaced with the Editor's alternative; and 27% prefer other solutions. Although no plan is supported by a majority of Associates, the present symbol seems favored by more persons than any other proposal. This position contrasts sharply with the reactions reported in May, when 69% of those who replied wanted the list altered.

Some of the arguments presented

most frequently for retaining the present list were:

"There are too many 'vested interest' reactions of minuscule importance expressed thus far. I strongly urge you to leave the list alone until you have had time to give it more thought. Otherwise, you will have to change it annually. (U.S.A.)

Very definitely, leave the symbol as it is, since its present form appeals to more persons from borderline fields. (Germany)

The term *anthropology* is not understood everywhere as being synonymous with "the sciences of man." In the scientific circles I know (France and the Argentine), the legend "Cultural, Social, Historical, and Biological Anthropology" would prove less comprehensive than that which now accompanies the cover-symbol. In these countries, for example, linguistics is a science of man, but it is not a division of anthropology. The point is that CURRENT ANTHROPOLOGY ought to become a clearing-house for all the sciences of man, to be a unique and very useful journal. The legend the Editor proposes ill serves this end, it seems to me. (France)

The omission of linguistics, in fact, was the most frequently expressed criticism of the proposed substitute.

Linguistical work is too often overlooked as part of cultural, social, and historical anthropology to be omitted by name. (U.S.A.)

Linguistics is a recognized, well-defined branch of anthropology, and is not a specific part of any of the four branches named. (U.S.A.)

Those who preferred the substitute generally signified their views without setting forth reasons for their choice.

The alternatives, for the most part, were variations on the original list, or on the proposal advanced by the Editor; and presented mostly views covered in the May report. The omission of Cultural Anthropology from the original list still evokes much criticism, as does, in a lesser degree, the omission of Ethnology from the substitute. It is now clear that many Associates, including several social anthropologists, consider Social Anthropology a part of Cultural Anthropology and will not accept its status as a separate sub-discipline under any circumstances. The most frequently voiced criticism of the original list in these recent replies was the inclusion of Prehistory as well as Archaeology.

If the replacement of sub-fields by broader categories is approved by only about one-quarter of the Associates, other alternatives attracted even fewer

supporters. Seven persons (3.5%) desire a motto, with three of these favoring "*Nihil humani a me alienum puto*"; on the other hand, three Associates specifically objected to any motto. Eight Associates prefer no words at all, and two want only symbols.

Two Associates, one an adherent of the present list, and the other of the proposed substitute, offered perspicacious advice:

It is an impossible task to try to satisfy even a quarter of the Associate group on this point. (U.S.A.)

. . . You will never get agreement on what our subdisciplines are. . . (U.S.A.)

I have read the letters from Associates in the first three numbers of CURRENT ANTHROPOLOGY with great interest. They contain the makings of what might be an ethnology of anthropologists.

As to the cover symbol: I doubt that one can ever be developed that will please everybody. Some of the objections, such as to the largeness of Antarctica, suggest an excessive literal-mindedness. Physical Anthropology, Archaeology (and I prefer that spelling), Ethnology, and Linguistics are old-established terms commonly used to denote recognized subdivisions of the discipline. I see no point in trying to find new terms for them; and arguing that Linguistics is a part of culture and hence of Ethnology is too fine-drawn, or legalistic. I think there is some difference between Prehistory and archaeology, but if there are to be changes or omissions of the terms used, Prehistory, Social Anthropology, and Folklore are those most open to omission or replacement.

Ethnography, to me, is a term to be included. I have remarked elsewhere that a large part of anthropology could be basically divided into Ethnology and Ethnography. The latter is the direct study, usually in the field, of phenomena within a specified locus, usually a single culture or a unit such as a tribe. The former utilizes these studies to form hypotheses and conclusions, hopefully of world-wide application. Social and Cultural Anthropology are approaches and methods within these two older terms; either may be used in simple Ethnography, in Ethnology, or in both.

OLIVER LAFARGE

Radiocarbon Dates and Upper Palaeolithic Archaeology in Central and Western Europe¹

by *Hallam L. Movius, Jr.*

THE PRIME IMPORTANCE to archaeology of an objective method for counting time cannot be overstated. Since archaeology alone makes possible the extension of our knowledge of man's early attempts to develop civilization on this planet beyond the limits of written records, it is an historical science. As such it depends on dates that are as accurate as possible to serve as a basis for the establishment of given regional chronologies. This is particularly true of Palaeolithic archaeology, a field in which for nearly a century the fundamental problem has been *time*: how to align the hundreds of sites, with their occupation layers, soils, faunal material, human remains, and thousands of artifacts, in an orderly

chronological scheme. Without such a framework the over-all picture becomes confused and, in certain instances, almost meaningless. Time alone is the lens that can throw it into focus.

Prior to 1950, however, the commonly available archaeological chronologies for prehistoric events could be established only on a relative basis. For this reason they lacked precision and were often proved to be badly in error. Fortunately an orderly cognate science, atomic physics, has given us the much needed tool, radiocarbon dating. From the beginning, this method provided archaeologists with a new research device that already has made invaluable contributions to archaeology. But like all fine tools this one must be kept sharp, and handled with care and perception: the most promising occupation site cannot possibly be expected to yield results without the brain, hand, and eyes of the excavator.

Thus far several thousand radiocarbon determinations have been made on archaeological samples from all over the world, and the basis for a chronology for the last 60,000 or so years is beginning to emerge. Furthermore, for the early lithic assemblages from sites that were occupied during the Last Glaciation in Europe and North America, it is now possible by a single method to collate chronologies independently established in several allied fields. This objective method of solving chronological problems which are of an interdisciplinary nature is of fundamental importance, especially with respect to materials of Late Pleistocene age. However, the relevant dates for scattered European localities covering the last sixty millennia which have been provided thus far by our physicist colleagues appear, on first glance, to add up to a picture that is less than crystal clear. One reason for this is that there are

HALLAM L. MOVIUS, JR., is Professor of Anthropology in the Department of Anthropology, and Curator of Palaeolithic Archaeology in the Peabody Museum, of Harvard University (Cambridge, Massachusetts, U.S.A.). Born in 1907, he was educated at Harvard (A.B., 1930; Ph.D., 1937). His dissertation on "The Irish Stone Age" was published in 1942 by the Cambridge University Press. His total scientific bibliography comprises more than seventy items.

MOVIUS has done archaeological work on various aspects of the Old World Palaeolithic and Mesolithic cultures in a wide range of areas: Czechoslovakia (1930), Western and Central Europe (1931), Palestine (1932), Ireland (1932-36), Burma and Java (1937-38), Eastern France (1948), Western Europe (1949), and Southwestern France (1953 and 1958-60). At present, he is engaged in excavating a large Upper Palaeolithic rock shelter, known as the Abri Pataud, which is situated in the village of Les Eyzies in the classic Dordogne region of Southwestern France. Certain significant aspects of the research conducted to date at this important site are discussed in the present paper; additional information is provided in several of the items listed on page 390 of the bibliography.

The present article, submitted to *CURRENT ANTHROPOLOGY* on April 12, 1960, was sent for CA☆ treatment to twenty-four scholars, of whom the following responded with written comments: Martin Almágro, Alberto C. Blanc, François H. Bordes, Hugo Gross, Sheldon Judson, Vojen Ložek and Jiří Kukla, Karl-Otto Münnich, László Vértess, Paul Woldstedt, and H. E. Wright, Jr. The comments written for publication are printed in full after the author's text, and are followed by a reply from the author. References cited by a commentator but not by the author are inter-alphabetized with the author's bibliography but distinguished by the initials of the commentator.

¹ The writer is particularly grateful to Dr. Hugo Gross of Bamberg (Bavaria) and to Professor François H. Bordes of the Université de Bordeaux for their advice and very helpful comments during the preparation of this paper. He also wishes

all too few determinations at present; another is that the very sites themselves have not always been studied with uniform care and precision. Only if the archaeologists and geologists do their part of the job as thoroughly as the physicists have done theirs, by examining in detail the total content of each horizon so dated, can we use this new and wonderful tool properly. Future research along these lines on a carefully determined regional basis will inevitably provide fresh and valuable information that will make possible the extension of the palaeo-geographical or environmental approach to the interpretation of archaeological materials from sites that were occupied some 60,000 to 10,000 years ago. Part I of this paper summarizes the chronological results achieved thus far with reference to the Fourth Glacial (Würm/Weichsel) Stage, while in Part II an attempt has been made to interpret the classic Upper Palaeolithic sequence of Southwestern France in terms of this chronology and in the light of the available C-14 dates.

I. CENTRAL AND NORTHERN EUROPE

FOURTH GLACIAL CHRONOLOGY

In Europe a fairly reliable and consistent absolute chronology for Fourth Glacial (Würm/Weichsel) time, based on approximately 120 radiocarbon dates, is beginning to emerge. The following summary is in large measure a synthesis of the publications of Brandtner (1950, 1954, 1956), Fink (1954), and Prošek and Ložek (1954, 1957) with respect to the loess stratigraphy of Central Europe, and of Gross (1954, 1955, 1956, 1957a, 1957b, 1958, 1959) and Woldstedt (1950, 1954, 1955, 1956, 1958) dealing with various aspects of the North European sequence, the over-all correlation of Fourth Glacial events in Northern and Central Europe as a whole, and the C-14 dating evidence bearing thereon. These results are set forth in Figure 1. Although many problems remain to be solved, the prime importance of this chronology for an understanding of Upper Palaeolithic cultural development and its bearing on those archaeological events which have taken place since the Last Interglacial cannot be overstated.

No attempt will be made to suggest a correlation of the Würm sequence of the Northern and Western Alps with that of Northern and Central Europe. Indeed the whole problem of the alleged subdivisions of the Fourth Glaciation of the Alpine region is far from satisfactory solution, as recent discussions by Judson (with Movius,

1956: 70-71) and Wright (1957: 456-58) have clearly indicated. For this reason many European Pleistocene geologists have abandoned the terms "Würm I," "Würm II," and "Würm III" with reference to subdivisions of the Last Glaciation, as has been advocated by Büdel (1950, 1953), Gross (1957b: 20-30; 1958; 1959), and Woldstedt (1954, 1956, 1958). Instead, the terms "Early Würm" (*Altwürm*), "Middle Würm" (*Mittelwürm*)² and "Late Würm" (*Spätwürm*) are employed.

It seems apparent that a second terminological matter is also in need of clarification. This concerns the intervals of climatic amelioration which provide the basis for subdividing the Last Glaciation and for correlating regional sequences. At present two terms—"interstadial" and "oscillation"—are rather loosely used in this connection. Accordingly, in the interests of a more refined and realistic terminology, the following definitions, both of which have been adhered to in this paper, are offered:

The term *interstadial*, as employed by the present writer, designates an interval of climatic amelioration between two stadials of the same glaciation of the Northern Hemisphere, during which, on the basis of the pedological, paleontological, and paleobotanical evidence, a sub-arctic to cool temperate (boreal) climate prevailed in many areas, which was cooler than the present one even at the time of its optimum. Although interstadial conditions may have persisted in a given region for many thousands of years, the ice-sheets did not recede to their present limits, which would certainly have been the case during a true interglacial, when the climate was at least as warm as that during the Post-Glacial optimum. As Gross (*in litt.*, 21.1.58) has observed, an interstadial is further characterized by weak to medium intensity of soil weathering, the restricted occurrence of predominantly coniferous forests in favorable regions, and by a relative increase in the frequency of characteristically forest-type animals.

An *oscillation*, on the other hand, as the term is used in this paper, denotes an interval of shorter duration than an interstadial, an interval which is characterized by a temporary climatic amelioration. This is evidenced by comparatively feebly-developed soil weathering processes, little if any significant faunal change, and the occurrence of sparse coniferous forests only in very limited areas, such as valley bottoms and maritime lowlands. During an oscillation the normal movement of the ice-sheets was only temporarily interrupted—i.e., an advance movement was arrested, or a movement of retreat was accelerated. The glacial episodes separated by an oscillation are *not* regarded as true stadials.

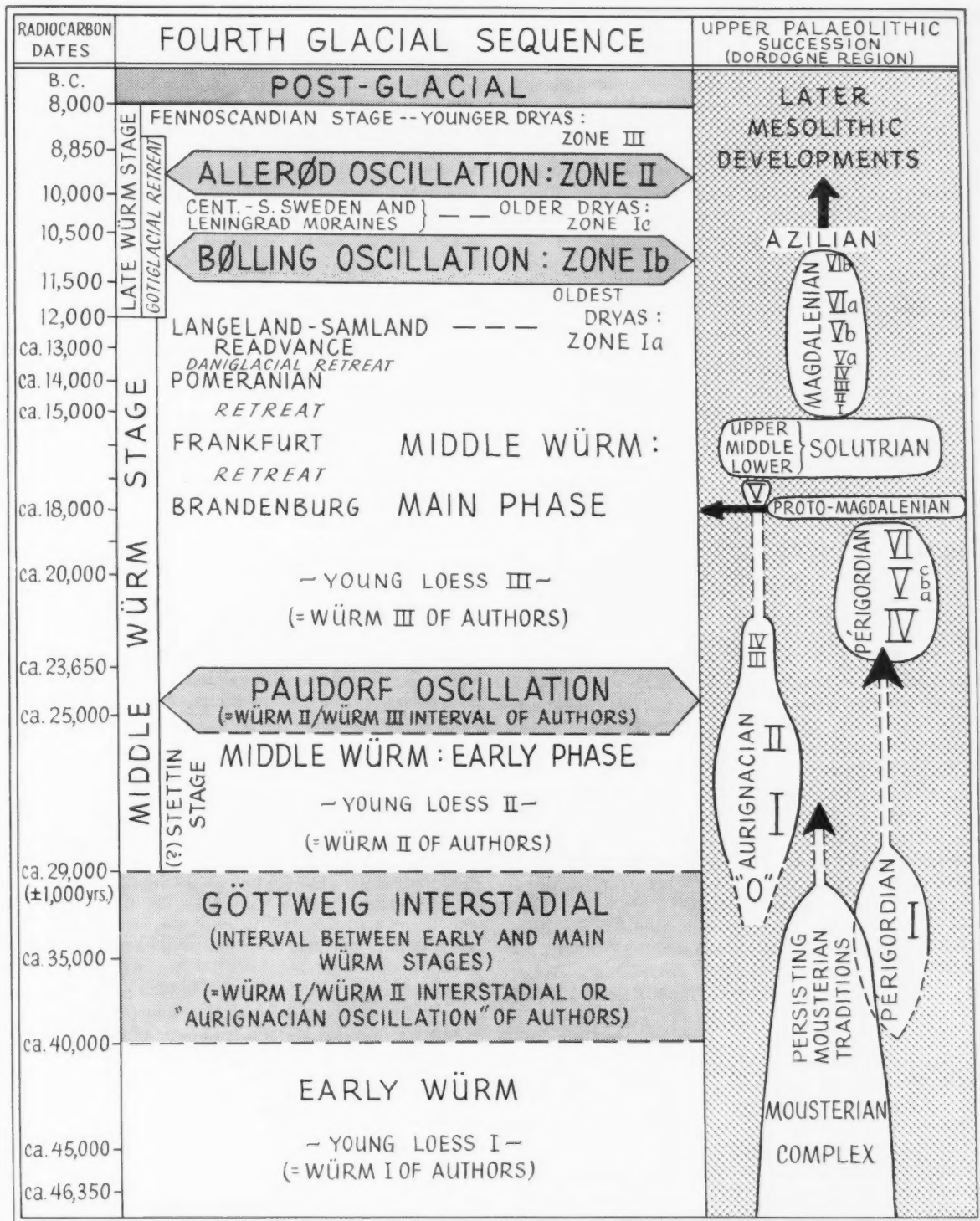
The first two major subdivisions of the Würm Glaciation are separated by an interstadial—the Göttweig—when polleniferous deposits were accumulated at several localities in Northwestern Germany. According to Selle (1952), the diagrams show that there was an initial dominance of birch (*Betula*) followed by pine (*Pinus*), which suggests that this was a true interstadial on the basis of the definition given above. Although it is unknown where the Early Würm Ice stood at the time of the maximum advance, the limits during the Middle Würm, which is separated into two phases by the short, mild Paudorf Oscillation, are clearly marked stratigraphic units. It is believed that the subsequently over-

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²Gross follows Büdel in using the term "Main" or "High" Würm (*Hauptwürm*); Woldstedt advocates "Middle" Würm (*Mittelwürm*), which, on the whole, seems more consistent.

FIG. 1. Radiocarbon-dated Würm chronology and the Upper Palaeolithic sequence in Western Europe.

Movius : UPPER PALAEO-LITHIC RADIOCARBON DATES



ridden Stettin moraines of Northern Germany belong to the early phase of the Middle Würm. Following the Paudorf interval, the maximum advance, registered by the Brandenburg moraines, was attained. The climate at this time was extremely cold and dry, which was like-

wise the case during the ensuing substages: the Frankfurt, Pomeranian, and Langeland-Samland, respectively. Nothing is known of the climate during the retreat intervals separating these latter episodes, but the morainic deposits of each have been studied in detail.

The beginning of the retreat of the Fennoscandian Ice-Sheet following the Langeland-Samland Re-advance defines the lower boundary of the Gotiglacial, which in turn marks the beginning of the Late Würm Stage. This latter interval ended *ca.* 8,000 B.C. with the beginning of Post-Glacial time. The factual evidence on the basis of which this chronology has been established, including the radiocarbon dates pertaining thereto, is summarized below. This chronology is of direct and fundamental importance to Palaeolithic archaeology; for the first time it provides a solid framework for understanding the classic Upper Palaeolithic sequence in South-western France, as discussed in Part II of this paper.

A. EARLY WÜRM

The oldest radiocarbon date thus far determined for an Old World locality was announced in 1958 by the Groningen laboratory for a sample of wood from the well-exposed profile at Amersfoort in the Eem Valley (Netherlands) and described by De Vries (1958: 12). This sample (GRO-1397), designated "Amersfoort XII," was subjected to a special new isotopic enrichment process, and its age was determined as 64,000 B.P. ("before present") $\pm 1,100$ years (Haring, De Vries, and De Vries 1958). It is believed that this wood dates from an Early Würm oscillation separated from the Last (Riss/Würm) Interglacial by a cold period. On this basis, it is probable that the Early Würm Glaciation had already gotten under way by 70,000 B.P., but the absolute age of the Last (Eemian) Interglacial, which is registered in the Eemian deposits as defined by Van der Vlerk and Florschütz (1953: 17-19), has not yet been established.

A second interval of unknown intensity and duration (? tentatively considered an interstadial) has also been recognized on the basis of samples from the Brørup Hotel Bog in Jutland (Andersen 1957) and from Loopstedt, near Schleswig in Northwestern Germany (Kolumbe 1955). Measurements of the Brørup peat performed in both the Copenhagen and Groningen laboratories showed that it is "older than 50,000 B.C." (Tauber and De Vries 1958). A comparable figure—GRO-1365: 50,000 B.P. $\pm 2,000$ years—was obtained for a sample from Loopstedt (De Vries 1958: 11). Now considerable controversy has arisen concerning the stratigraphic position of the so-called "Loopstedt Interstadial" with respect to the Würm sequence as a whole. Certain authorities (Andersen 1957; Narr 1959; Tauber and De Vries 1958; and others) have claimed that this horizon belongs to the beginning of the Göttweig Interstadial and accordingly marks the end of the Early Würm. However, in the absence of anything approaching factual evidence in support of such a view, this interpretation is considered very unlikely (compare Gross 1958: 164; Woldstedt 1958: 153). According to Florschütz (1957: 246), this interstadial is not registered in the Tubantian, or Pleniglacial, sequence of the Netherlands. On the other hand, the fact that a marked oscillation did occur in Czechoslovakia during the Early Würm is clearly shown at several loess sections (Prošek and Ložek 1954; Valoch, with Bordes, 1957), where a loamy zone has been observed between the soil horizons of the Last Interglacial (Riss/Würm) and the Göttweig Interstadial. This zone subdivides the Early

Würm loess into two units designated Ia and Ib. In Northern France, as pointed out on page 365, a comparable situation has been observed at a large number of localities.

Following the "Loopstedt Interstadial" came the third and final phase of the Early Würm. The climate at this time was humid and cool, as had been the case during previous Würm episodes. The precise limits of the ice-sheets at this stage are unknown. To the early part of the interval in question two localities, Lebenstedt and Senftenburg, may be assigned on the basis of the C-14 dating evidence (De Vries 1958: 13-14). The former is a Middle Palaeolithic locality and the oldest archaeological site in the Old World to be dated by the radiocarbon method. The relevant facts pertaining to each of these stations may be summarized as follows:

LEBENSTEDTGRO-1219: 48,300 $\pm 2,000$ yrs.
(46,350 B.C.)

Lebenstedt, near Salzigitter, north of the Hartz Mountains and southwest of Brunswick (Germany). Sample collected in the culture layer of this "cold" Mousterian (Mousterian of Acheulian tradition: Levalloisian facies) locality (Tode, Preul, Richter, *et al.* 1953; Tode 1954). On the basis of the geological evidence, Lebenstedt has been assigned to the beginning of the Early Würm, but the radiocarbon date shows that it is somewhat younger.

SENFTENBURGGRO-1217: 48,300 $\pm 2,000$ yrs.
(46,350 B.C.)

Senftenberg, near Krems on the Krems River, a tributary of the Danube in Lower Austria (Hampl 1950; Fink, with Felgenhauer and De Vries, 1959: 59-60). At this exposure, which has been intensively studied by Brandtner (1954), the Fellabrunn Soil Complex, formed during the Göttweig Interstadial (see below), clearly separates the Early Würm loess (Y.L. I) from the Middle Würm loess (Y.L. II). Although there seems to be some confusion concerning the precise stratigraphic position of this sample (compare Fink, with Felgenhauer and De Vries, 1959), Brandtner has informed the writer that the charcoal which he collected for measurement came from the upper portion of Young Loess I below the clearly defined Göttweig weathering horizon. The original date published by De Vries was the same as the Lebenstedt figure (48,300 B.P. $\pm 2,000$ yrs.), but De Vries (quoted by Fink, with Felgenhauer and De Vries, 1959: 60) subsequently felt that "contamination by rootlets could not be excluded. Therefore another part of the charcoal was subjected to a more elaborate chemical pretreatment. The final result is now:" GRO-1771: "more than 54,000 B.P."

Comment. It is possible that the Senftenberg charcoal-bearing horizon may be actually older than the "Loopstedt Interstadial" and that the geological evidence has been incorrectly interpreted. Whether or not this also applies to the Lebenstedt locality is not altogether clear. Gross (*in litt.*, 1.11.59) has informed the writer that the latter locality may also be "older than 50,000 years" on the basis of new measurements.

B. GÖTTWEIG INTERSTADIAL

The next subdivision of the Würm, an interval of interstadial magnitude (compare Gross 1956), is known as the Göttweig Interstadial. This marks the close of the Early Würm and separates it from the Middle Würm. In Lower Austria a well-developed fossil soil complex, known as the Fellabrunn Complex, was formed during this interstadial—the Laufen Schwan-

kung (Würm I/II) of the Penck-Brückner sequence.³ The Göttweig Interstadial is certainly older than the Brandenburg Stage of the North European sequence, as recently defined by Woldstedt (1955, 1956), but the position of the Stettin Stage with respect to this interstadial is not altogether clear. Both Woldstedt (1956) and Gross (1957b) originally suggested that the Middle Würm Stage of the Alps should be correlated with the Brandenburg, Frankfurt, and Pomeranian Substages of the Fourth Glacial (Weichsel) sequence of Northern Germany, their view being that the maximum advance of the latter occurred immediately subsequent to the Göttweig Interstadial. More recently, however, on the basis of the C-14 dates for the Paudorf Oscillation (see page 362), it has been pointed out that this event, as represented by the Brandenburg moraines, probably occurred approximately 20,000 or even 18,000 years ago, i.e., ca. 18,000 to 16,000 B.C. (compare Woldstedt 1958; Gross 1958), which would indicate a post-Göttweig age for the Stettin Stage. Although this view certainly seems to be more consistent with the field evidence, it is apparent that a series of radiocarbon measurements for samples of organic materials associated with these deposits would have a very important bearing on correlation studies of events of Middle Würm age. However, at a number of open-air and cave sites in Czechoslovakia, Lower Austria, and Hungary, occupation sites of Upper Palaeolithic hunters that belong to this and the following interval, on the basis of good stratigraphic evidence, have been reported (compare Brandtner 1956; Klíma 1957b; Pittioni 1954; Prošek and Ložek 1954, 1957; Schwabedissen 1956; Vértes 1955a). Although relatively few of these extensive Central European loess stations have been dated by the C-14 method, many of them have provided excellent charcoal samples which are available for radiocarbon analysis.

The Göttweig Loam Horizon (*Verlehmungszone*) which was formed on the Early Würm loess (Young Loess I) during what Bayer (1927: 157) called the "Aurignacian Oscillation" (=Würm I/Würm II Interstadial of authors) represents near the type locality only the remainder of a formerly more extensive fossil soil formation, which was originally overlain by one or more horizons of humus, or black earth (chernosem), as revealed at several exposures to the east of Göttweig. But elsewhere these have been reduced to small remnants which are preserved locally. The formation of this loam, or fossil soil, was connected with the decalcification (loamification) of the upper stratum of the Early Würm loess (Y.L. I), on the unweathered surface of which it was developed. The fact that in the Göttweig region there are thin lenses of loess intercalated in the black earth overlying this loam suggests that this was a comparatively long period during which the climate was boreal or even sub-arctic, on the average, with cooling

temperatures and cold oscillations. Florschütz has informed the writer that he agrees with this observation on the basis of the occurrence of thin layers of silt in the Göttweig (Tubantian: Interstadial I) peats of the Netherlands. There, conditions were generally favorable to the development of park landscapes and scattered light forests (Florschütz 1957: 246). In Central Europe at the time of the optimum of this interstadial, conditions must also have been relatively temperate, but thus far very little intensive paleobotanical research has been conducted in this area. In the Göttweig horizon at the famous locality of Dolní Věstonice (Unter-Wisternitz) in Moravia, Schürumpf has identified pollen of willow, birch, pine, oak, linden, elm, hazel, and alder (Bohmers 1941a, b; Knor, Ložek, Pelíšek, and Žebera 1953: 41); although no frequencies are given, this evidence is indicative of relatively temperate conditions. Vértes' (1955b) brilliant analysis of the paleobotanical, mineralogical, and pedological materials from the contemporary cave deposits of Hungary further confirm this view. Therefore, it is suggested that the Göttweig represents a true interstadial, as contrasted with the relatively short oscillation, discussed on page 362, when the Paudorf Loam was formed.

The fact that the pedological evidence demonstrates the occurrence of well-developed soils of chernosem type at Göttweig exposures in the steppe regions of Central Europe has misled certain workers into correlating this horizon with the Last (Riss/Würm) Interglacial (see Gross 1959, for discussion). The solution to this problem is at once apparent when the evidence of the Molluscan fauna is considered, as Ložek (*in litt.*, 26.2.58; see also Ložek 1955) has pointed out. During the Göttweig Interstadial, steppe forms are characteristic (*Helicella striata*, *Chondrula tridens*, *Abida frumentum*, *Pupilla muscorum*, *P. triplicata*, *Vallonia costata*, *Vertigo pygmaea*), while in the vicinity of mountainous and hilly country there also occur several forest forms (*Euomphalia strigella*, *Cochlodina laminata*, etc.). In this horizon one never finds species that are typical of the warm, moist forest environment which is so characteristic of the Last Interglacial, and which has been reported in Czechoslovakia at several stratified localities in the soil underlying the Early Würm loess. Ložek states that several of the guide species of this interglacial fauna have actually been extinct in Central Europe since the onset of Würm Glacial times (e.g., *Helicigona banatica*, *Soósia diodonta*, *Aegopinella ressmanni*, *Discus perspectivus*). Associated with these extinct forms, there is a rich forest fauna that has been typical of the region ever since Mesolithic (Early Post-Glacial) times, but is unknown in the soil horizons associated with the loess of the Würm Glaciation. This evidence clearly proves that there is a very marked contrast between the prevailing environmental conditions of the Last Interglacial, on the one hand, and the Göttweig Interstadial, on the other, a fact which is further substantiated by the evidence of the new interglacial locality in the vicinity of Pavlov (Pollau), near Mikulov (Moravia), recently investigated by Ložek (1957). Whereas the Last Interglacial has a faunal and floral development similar to that of the Post-Glacial, with a wide development of

³ For additional information concerning the Göttweig Interstadial, see Gross 1957b: 21-24. In Czechoslovakia this horizon likewise constitutes an important stratigraphic boundary separating Young Loess I from Young Loess II (compare Prošek and Ložek 1957; Musil and Valoch 1955, 1956; Musil, Valoch, and Nečasný 1954; Lais 1951, 1954). Several years ago Fink (1954; see also Schwabedissen 1956) suggested that the Göttweig (WI/WII) and the later Paudorf (WII/WIII) Zones be called Stillfried A and B, respectively, but in order to avoid confusion the names used in this paper seem preferable.

forest cover, the Götting Interstadial has only a limited forest fauna and flora. Indeed, there is no reason to suppose that during the latter interval conditions were at all similar to those of the Post-Glacial interval.

Radiocarbon dates recently announced by De Vries (1959; with Felgenhauer and Fink, 1959: 71; see also Gross 1959: 72-73) give the absolute age and approximate relative duration of time represented by the Götting Interstadial. According to the C-14 measurements, this long, somewhat boreal but nonetheless cold interval began at least 42,000 years ago (ca. 41,050 B.C.) and came to an end around 30,000 to 29,000 B.P. (ca. 28,050 to 27,050 B.C.)—an interval of approximately 12,000 to 13,000 years duration. The samples which have provided the basis for these dates were collected at the classic exposure in the Oberfellabrunn brickyard, near Hollabrunn, 70 kilometers northwest of Vienna, and the results may be summarized as follows:

GRO-1901:	Basal portion of Young Loess II	31,600 ± 500 yrs. (29,650 B.C.)
GRO-1745:	Humus zone in upper part of the ca. 1.50-meter thick fossil soil layer.	37,600 ± 700 yrs. (35,650 B.C.)
GRO-1800:	Black humus (chernosem) layer containing lenses of loess.	42,700 ± 1400 yrs. (40,750 B.C.)
GRO-1740:	Humus zone immediately above weathered surface of Young Loess I	41,900 ± 800 yrs. (39,950 B.C.)
-----	Sample collected in decalcified loamy zone in uppermost portion of Young Loess I	Insufficient carbon for counting.

Comment. According to De Vries (1959: 87-88; with Felgenhauer and Fink, 1959: 71), all the samples from the Oberfellabrunn exposure contained rootlets, with the exception of the one collected in the loamy zone in the upper part of Young Loess I which unfortunately contained insufficient humus to obtain a date. Although every effort was made to remove these rootlets during the pretreatment of each sample, De Vries feels that the figures for GRO-1901 and GRO-1740 may be slightly in error, since the rootlet content of each was unusually high. Nevertheless the results of these investigations are extremely promising, and they demonstrate the feasibility of dating loess profiles in which fossil soils are exposed.

There seems to be no question concerning the fact that the sequence of fossil soils exposed at Oberfellabrunn was formed during the Götting Interstadial (compare Brandtner 1954: 58, Abb. 2, Fig. 1; Fink, with Felgenhauer and De Vries, 1959: 65-69, Abb. 7-9), when a relatively cool climate prevailed interrupted by colder oscillations during which the deposition of loess dominated. Accordingly, it is apparent that the beginning of the Götting Interstadial should be placed before 40,000 B.C., and this view is confirmed by the results of measurements of interstadial peat from Upton Warren, near Droitwich, Worcestershire (England), determined by De Vries (1958: 13), which are as follows:

GRO-595:	Upper peat from Upton Warren	41,500 ± 1,200 yrs. (39,550 B.C.)
GRO-1245:	Lower peat from same locality	41,900 ± 800 yrs. (39,950 B.C.)

The Upton Warren locality is described by Barendsen, Deevey, and Gralenski (1957: 910); the Yale laboratory measurement for the upper peat—Y-311A: > 38,350—indicates an age in excess of 40,000 B.P. and confirms De Vries' figure for sample GRO-595.

The date of approximately 29,000 B.P. for the close of the Götting Interstadial is based on samples collected at two localities: Glütschtal, near Thun in the Bernese Oberland (Switzerland), and Istállóskő Cave in the Bükk Mountains (Hungary). At the Glütschtal exposure, which is 600 meters above sea level, there is a lignite outcrop (*Schieferkohle*) that has been assigned to the "Würm I/II" or "Spiezer" (=Götting) Interstadial by P. Beck, who collected the sample that was recently measured at the Bern laboratory (Oeschger, Schwarz, and Gfeller 1959: 138-39) with the following result:

B-20:	Interstadial lignite from Glütschtal	29,000 ± 1,500 yrs. (27,050 B.C.)
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At the Istállóskő Cave, which is situated at an elevation of 535 meters above sea level, two culture layers were recognized, both of which have been assigned to the Götting Interstadial on the basis of the paleontological, pedological, and other evidence (Vértes, Horusitzky, Korek, Malán, *et al.* 1955; summarized by Delporte 1957). The lower occupation, described as "Aurignacian I" by Vértes, is clearly separated from the upper "Aurignacian II" level by sterile deposits. In this connection, it should be pointed out that Vértes' so-called "Aurignacian II," characterized by bone points of Mladeč (Lautsch) type, is very similar to the assemblage originally described as *Olschewian* (Olschewa-Kultur) by Bayer (with Brodar, 1928: 9) from the Potočka Cave in Northern Yugoslavia (see below). On the basis of the published illustrations, it would appear that Vértes' "Aurignacian I" represents an earlier manifestation of the same development. The problem of the alleged "Aurignacian" of Central Europe is in urgent need of further intensive study. The radiocarbon dates for this site are given below:

GRO-1935:	Charcoal sample from upper occupation layer at Istállóskő Cave (Vértes 1959; with De Vries, 1959)	30,710 ± 600 yrs. (28,760 B.C.)
GRO-1501:	Charcoal from the lower level at the same locality (De Vries 1958:15)	30,670 ± 500 yrs. (28,720 B.C.)

Comment. It is very probable that the sample from the lower level was "contaminated," since in a letter to Gross (dated Budapest 12.15.57) Vértes stated that this lot of charcoal was packed in cotton wool at the time of the excavation in 1949-1950. On this basis, one is perhaps justified in concluding that the Upper Palaeolithic in Hungary is certainly older than 30,000 B.P., and that it was already well developed prior to the end of the Götting Interstadial. Vértes' (1959) estimate of ca. > 36,000 B.P., however, seems somewhat excessive.

The archaeological evidence suggests that Potočka Cave, situated at an elevation of 1,700 meters above sea level on Mt. Olsheva (Olschewa) in the Eastern Karawanken Range of the Southeastern Alps of Slovenia (Northwestern Yugoslavia), belongs to the same broad chron-

ological horizon as the upper level at Istállós-kő. This important site, which has been intensively studied by Brodar (1938: 149-59; 1939; 1957: 147-53; with Bayer, 1928; see also Zotz 1951: 200-201), is the type locality for Bayer's Olschewian Culture, as stated above. On the basis of the geological and pedological evidence (Lais 1941: 97-98), the early Upper Palaeolithic occupation of this site occurred during the Göttweig Interstadial, when the Southeastern Alps must have been ice-free up to an approximate altitude of 2,000 meters. According to Lais (1941: 97-98) the interstadial deposits at Potočka zijalka were accumulated during a time of intense weathering under conditions of a continental climate with a fairly severe winter and a complete local melting of the ice and snow during the summer. It is to be regretted that the charcoal samples collected at the time of the Potočka excavation were lost during World War II.

A second Alpine cave site, Salzofenhöhle, confirms the conclusions based on the Potočka evidence. This locality, situated in the Toten Gebirge, near Bad Aussee (Austrian Alps), at an elevation of 2,008 meters above sea level, was investigated by Ehrenberg (1953, 1956, 1957, 1959), and the paleontological data suggest an interstadial dating. Indeed Schmid (1957) states that in her opinion the charcoal-bearing level at Salzofenhöhle belongs to the Göttweig Interstadial, on the basis of her granulometric analysis of the cave sediments, a conclusion which was subsequently confirmed by De Vries' and Waterbolk's (1958: 1553) C-14 date for one of Ehrenberg's samples:

GRO-761: Charcoal from the main layer at Salzofenhöhle with Cave Bear remains plus a few atypical stone artifacts belonging to the so-called "Alpine Palaeolithic." 34,000 \pm 3,000 yrs.
(32,050 B.C.)

This evidence shows that the Austrian Alps were also free of ice up to an altitude in excess of 2,000 meters during the Göttweig Interstadial. Additional radiocarbon dates for samples collected at localities which may be assigned to this interstadial are given below:

FLADBURY GRO-1269: 38,000 \pm 700 yrs.
(36,050 B.C.)

Fladbury, near Birmingham (England). Peat sample of interstadial age (De Vries 1958: 13).

FARMSUM GRO-1279: 37,900 \pm 1,000 yrs.
(35,950 B.C.)

Farmsum, 40 kilometers northeast of Groningen (Netherlands). Sample II: Upper peat; De Vries (1958: 12) considers that the date of Sample I: Lower peat is uncertain.

AMERSFOORT GRO-1259: 34,730 \pm 700 yrs.
GRO-1276: 34,730 \pm 500 yrs.
(32,780 B.C.)

Amersfoort, Eem Valley (Netherlands). Sample XI: Peat (De Vries 1958: 12).

BREDA GRO-936: 32,000 \pm 900 yrs.
(30,050 B.C.)

Breda, North Brabant (Netherlands). Peat sample measured by De Vries, Barcnden, and Waterbolk (1958: 130), which was collected at a depth of 2.80 to 3.00 meters, overlain by alternating layers of peaty material and loess mixed

with sand. According to Van der Hammen (1951), the pollen diagram points to a Pleniglacial age.

Actually the close of the Göttweig Interstadial is somewhat difficult to define in terms of any single round figure, such as a terminal date of 29,000 B.P., which is currently quoted for this alleged "event." But one must not lose sight of the fact that there is a probable error factor involved here of the order of 1,500 years, e.g., the figure for the Glütschtal lignite (B-20: 29,000 \pm 1,500 yrs.). Indeed a very reasonable case can be made for accepting a date of approximately 31,000 B.P., or even slightly older, on the basis of the measurements that are available at present, and in this connection it is perhaps appropriate to point out that the figure 31,600 B.P. \pm 500 yrs. (GRO-1901) was determined for a sample collected by the late Professor De Vries himself at Oberfellabrunn in the basal portion of Young Loess II overlying the upper humus zone, which, as stated on page 359, was formed during the Göttweig Interstadial.

C. MIDDLE WÜRM: EARLY PHASE

The fact that the upper limits of the Göttweig Interstadial (if, indeed, one is justified in assuming that such a stratigraphic boundary actually does exist) have not yet been precisely defined, is clearly shown by the following series of radiocarbon dates for the famous locality of Willendorf in the Danube Valley of Lower Austria (Kromer 1950; Brandtner 1955; Felgenhauer 1959; Fink, with Felgenhauer and De Vries, 1959: 57-58 and Abb. 3). Here there occur a series of Upper Palaeolithic occupation levels stratified in the basal portion of Young Loess II and accordingly of Middle Würm: Early Phase age. Felgenhauer (1959: 201), who has published the Heidelberg date for the first time, states that sample GRO-1287 was collected during the 1908 investigations, whereas samples GRO-1273 and H-246/231 were recovered during his own 1955 excavations. The results (De Vries 1958: 14-15; Felgenhauer 1959: 201) may be summarized as follows:

GRO-1273: Willendorf II: charcoal from hearth in Layer 4, an horizon which is stratified in the lower portion of Young Loess II and accordingly is believed to be of early Middle Würm age.
..... 31,840 \pm 250 yrs.
(29,890 B.C.)

Comment. Due to a misunderstanding of the stratigraphic evidence, the present writer has incorrectly stated elsewhere (Mövius 1957: 248) that Willendorf II/4 should be assigned to the very end of the Göttweig Interstadial.

H-246/231: Willendorf II: charcoal from hearth in Layer 5, which immediately overlies Layer 4 (see above) 32,000 \pm 3,000 yrs.
(30,050 B.C.)

GRO-1287: Willendorf II: charcoal from hearth in Layer I, a thin occupation layer in the basal portion of Young Loess II 30,310 \pm 250 yrs.
(28,360 B.C.)

Comment. It is difficult to understand why the lower and doubtless somewhat older sample collected in 1908 in Layer I should be ca. 1,500 years younger than the figure for the overlying Layer 4. Brandtner (in Felgenhauer 1959: 195)

feels that this discrepancy cannot be explained simply as being due to the natural tolerance of the C-14 method, and, consequently, that it is the result of contamination of the sample.

It seems to the present writer that, on the basis of the evidence summarized above, it is somewhat misleading to insist that the close of the Göttweig Interstadial has been established at *ca.* 29,000 B.P. (*ca.* 27,000 B.C.). Indeed, as Woldstedt (1958: 153) suggests, one can date the end of this interval at around 28,000 to 30,000 B.C. It is therefore felt that perhaps it would be more in keeping with the known facts to use an approximate figure of 31,000 B.P. \pm 1,000 years (29,000 B.C. \pm 1,000 years), at least until further work has been done in an effort to resolve this problem. In the meantime, two dates which fall in the early phase of the Middle Würm, an interval that corresponds to Florschütz's (1957: 246) Tubantian: Pleniglacial B., should be considered:

EMMELOORDGRO-390: 29,000 \pm 5,000 yrs.
(27,050 B.C.)

Locality near Emmeloord, North East Polder (Netherlands). Hypnum peat of pleniglacial age at a depth of 8.00 to 8.20 meters (De Vries, Barendsen, and Waterbolk 1958: 130). In view of the magnitude of the error (5,000 yrs.), this measurement is of little value for chronologic studies.

WOUWGRO-931: 28,500 \pm 540 yrs.
(26,550 B.C.)

Locality near Wouw, North Brabant (Netherlands). Lower part of peat layer of a pleniglacial age at a depth of 1.00 to 1.20 meters (De Vries, Barendsen, and Waterbolk 1958: 130).

D. PAUDORF OSCILLATION

At the Central European loess stations, a second fossil soil, known as the Paudorf Loam, overlies Young Loess II and separates it from a third horizon (III) of the Young Loess. According to Brandtner (1950: 104; 1954), its morphological characteristics indicate that climatic conditions during the Paudorf interval were considerably more continental than had been the case during the Göttweig Interstadial. Since the paleontological and paleobotanical remains from Upper Palaeolithic sites associated with the Paudorf horizon in Central Europe represent both forest and cold-adapted animals, as well as charcoal of coniferous trees, it is concluded that the then-existing climate was very similar to the present climate in the northernmost portion of the taiga zone of Eurasia and North America, with its prolonged winter and relatively short summer. Presumably patches of true forest existed only in the valley bottoms, while sub-arctic conditions prevailed in the uplands. According to the definitions proposed in this paper (see page 356), the Paudorf horizon represents an oscillation. Certainly it is not a true interstadial; rather, it is indicative of a very temporary improvement in the climate during the Middle Würm, subsequent to which the maximum advance of the last ice-sheets occurred. Florschütz (1957: 246) states that in the Netherlands at this time there was a temporary return of parkland conditions, and even the local development of forests with thermophilous elements, including the presence of *Carpinus* (Hornbeam). However, in Central Europe, the cold, dry conditions indicative of a continental climate prevailed.

In marked contrast to the 12,000- to 13,000-year-long Göttweig Interstadial, the Paudorf Oscillation covered a relatively short interval, possibly not over 2,000 years long, on the basis of the radiocarbon dates determined thus far—GRO-931: 28,500 \pm 540 years (*ca.* 26,550 B.C.) for the early phase of the Middle Würm at Wouw (see above), and GRO-1286: 25,600 \pm 170 years (*ca.* 23,650 B.C.) for the upper portion of the Paudorf fossil soil horizon at the Eastern Gravettian site of Dolní Věstonice in Southern Moravia. Thus only one reliable sample from the Paudorf horizon has been measured; furthermore, as De Vries (1958) has stated, no dates marking the beginning of this oscillation are yet available. A single sample from Geesthacht, near Hamburg (Germany), is believed to be of Paudorf age on the basis of the C-14 evidence. The data on which the present chronology is based (De Vries 1958: 13-14) are summarized below.

DOLNÍ VĚSTONICEGRO-1286: 25,600 \pm 170 yrs.
(23,650 B.C.)

Dolní Věstonice (Unter-Wisternitz), near Mikulov, Southern Moravia (Czechoslovakia). Charcoal from hearth in Eastern Gravettian occupation layer situated in the uppermost portion of the Paudorf horizon (De Vries 1958: 14; Klíma 1950, 1952, 1954, 1957b: 115-18, 1957c: 137-39; Knor, Ložek, Pelišek, and Žebra 1953; Lais 1954).

Comment. The same date, i.e., 25,600 B.P. \pm 100 years (GRO-1327) has also been announced by De Vries (1958: 14) for sample *a* from Aggsbach, near Melk a. d. Donau in Lower Austria (cf. Felgenhauer 1951). Sample *b* (GRO-1354) gave a figure of 25,540 B.P. \pm 170 years. However, according to Brandtner (personal communication), this is not possible due to the fact that the occupation layer in question occurs near the base of Young Loess II and accordingly is approximately the same geologic age as Willendorf II/4 (see page 361). Presumably this sample was "contaminated" by recent material introduced either by tree roots or rodent burrows, or both, although De Vries (1958: 14) states that hardly any rootlets were present either at the site or in the sample.

GEESTHACHTGRO-1515: 26,600 \pm 300 yrs.
(24,650 B.C.)

Geesthacht, near Hamburg (Germany). Sample III: Loamy sand with humus (De Vries 1958: 13).

The view that the Paudorf Oscillation ended about 23,000 B.C. is supported by the date obtained for a second large Eastern Gravettian station in Moravia—Pavlov, also near Mikulov and in the immediate vicinity of Dolní Věstonice. According to Klíma (1957c: 139), the occupation layer at Pavlov still belongs to the time of cool climate before the establishment of true Middle Würm periglacial conditions, characterized by various types of cryopedological phenomena. The date (De Vries 1958: 14) is given below:

PAVLOVGRO-1325: 24,800 \pm 150 yrs.
(22,850 B.C.)

Pavlov (Pollau), approximately 500 meters south of Dolní Věstonice. Charcoal from hearth in Eastern Gravettian occupation layer situated just above the Paudorf fossil soil (De Vries 1958: 14; Klíma 1955, 1957a, 1957b: 118-26, 1957c: 139, 1958, 1959; Musil 1955).

E. MIDDLE WÜRM: MAIN PHASE

Recently both Woldstedt (1958) and Gross (1958) have expressed the opinion that the Paudorf interval represents a weak oscillation during the re-advance of

the Würm/Weichsel Ice to the maximum—an event which occurred approximately 18,000 B.C., or possibly even later. Therefore, it is very likely that the subsequently overridden Stettin moraines were formed during the early phase of the Middle Würm Glaciation, which marked the end of the Göttweig Interstadial. On the other hand, as Woldstedt points out (*in litt.*, 5.1.60), although direct evidence is lacking, it is possible that the Stettin moraines belong to the later (main) phase of the Middle Würm, shortly before the maximum. In any case, the limits of the maximum advance in Northern Europe are clearly defined by the Brandenburg moraines of North Germany, which were formed at a time when the climate was extremely cold and dry. However, as stated on page 359, no organic materials associated with these extensive deposits have ever been submitted for C-14 measurement. And the same observation also applies to the subsequent Frankfurt and Pomeranian Stages. In the exposures of Young Loess III at the Central European localities none of these events is registered, unless the three relatively minor Post-Paudorf oscillations (or *intermikrostadials*) described by Klima (1957b: 88, Bild 2; 1957c: 133, Fig. 12) as being present in the Youngest Loess (Y.L. III) in the region of the Pollauer Berge of Southern Moravia are to be correlated with the short intervals of glacial retreat which occurred between each of these stages. Broad-scale correlations of this nature can only be verified when radiocarbon chronologies have been established for each of these regional sequences.

The Daniglacial phase of the North German-Scandinavian sequence occurred during the retreat of the ice from the Pomeranian to the Langeland moraines, in accordance with the very plausible definition of the term "Daniglacial" proposed by Wright (1957: 454). Apparently, complete uninterrupted pollen diagrams have not been extended back as early as the Daniglacial/Gotiglacial boundary, but nevertheless a definite climatic change occurred at this time, which marks the beginning of the Late Glacial (Late Würm) Stage. According to Gross (1954: 198), the oldest diagrams begin in the closing phase of the treeless tundra zone—the Oldest Dryas, or Zone Ia in the North European pollen diagrams. Here the Langeland Advance, as defined by Woldstedt (1929: 210; 1950: 369–70), may be placed, and doubtless the Samland end-moraine of East Prussia as well (Gross 1954), since the retreat from the latter also marks the beginning of the Gotiglacial. The paleobotanical evidence and C-14 dating of two late Upper Palaeolithic sites, both near Ahrensburg in the Hamburg region of Holstein (Northwestern Germany), have an important bearing on the chronology of this interval. These dates are as follows:

MEIENDORF W-172: 15,750 \pm 800 yrs.
(13,800 B.C.)

Meiendorf, near Ahrensburg and approximately 13 kilometers northeast of Hamburg. Sample of gyttja from the type site of the Hamburgian I culture (Rubin and Suess 1955: 473; Rust 1937, 1943, 1951a: 207–208).

POGGENWISCH W-93: 15,150 \pm 350 yrs.
(13,200 B.C.)

Poggenwisch, between Meiendorf and Ahrensburg, 15 kilometers northeast of Hamburg. Sample of gyttja from the

Hamburgian II culture layer. Stratigraphically this site is slightly later in the Oldest Tundra than Meiendorf (Suess 1954: 487–88; Rust 1951b: 50, 1951c; Schütrumpf 1955: 47–50).

Schütrumpf (in Rust 1937; Schütrumpf 1955: 47–50) analysed the pollen diagrams at both sites, and according to him each of them belongs to the Oldest Dryas phase (Zone Ia), with the Poggenwisch locality being slightly younger since the diagram already shows changes that indicate the approach of somewhat less severe climatic conditions. It is believed that the latter are to be correlated with the beginning of the retreat of the ice-sheet from the Pomeranian to the Langeland moraines, and not with the post-Langeland Bølling Oscillation, as suggested by Schütrumpf. With regard to the date of the beginning of the Daniglacial, in the bog section at Heiligenhafen, north of Lübeck (Gross 1954: 194–95; 1955: 111), Oldest Tundra and Bølling deposits occur on top of the main Pomeranian moraine, demonstrating that this feature is at least 15,750 years old (*ca.* 13,800 B.C.) on the basis of the Meiendorf measurement. This conclusion is substantiated by the fact that at Grömitz, on the southeast shore of Lübeck Bay and south of Heiligenhafen, unrolled Hamburgian II artifacts have been reported by Brückner (1954) deeply imbedded in morainic deposits believed to have been accumulated during the late part of the Pomeranian Stage. Apparently at this locality a Reindeer-hunter's temporary camp site was overridden by a re-advance of the ice during the Oldest Dryas phase. As Schütrumpf (1955: 49–50) has stated, this evidence suggests that the main Pomeranian Stage is at least 15,150 years old (*ca.* 13,200 B.C.), although Gross (1955: 111) points out the possibility that the Grömitz artifacts may have been introduced secondarily into the ground moraine by cryoturbation rather than by overriding by the glacial ice.

F. LATE WÜRM⁴

Iversen (1942) was the first to recognize the Bølling Oscillation, when he found clear evidence for a mild interval in the middle portion of Zone I of the pollen diagram for Bølling-Sø in Denmark, and on this basis he subdivided Zone I of the Late-Glacial sequence of Denmark into sub-zones *a*, *b* and *c*. Subsequently the occurrence of this oscillation was established at other localities in Northern Germany, the Netherlands, and, recently, in Spain. In each instance it is separated from the overlying Allerød horizon by the Older Dryas deposits of Zone Ic, which is characterized by cryoturbation phenomena and tundra conditions. As both Gross (1954) and Schütrumpf (1955) have shown, the Bølling belongs in the early part of the Gotiglacial Retreat; it is younger than the beginning of the withdrawal of the ice following the Langeland-Samland Re-advance.

Recent C-14 dates announced for peat samples of Bølling age collected at Lake Gatersleben, near

⁴ For further information concerning the radiocarbon-calibrated Late-Glacial chronology of Northwestern Europe briefly discussed in this paper, see Gross 1957a: 172, 1957b: 31; Barendsen, Deevy, and Gralenski 1957: 918–19 and Fig. 1; and Wright 1957. Pollen zones are given according to Iversen (1954) and Van der Hammen (1957a, b).

Aschersleben and south of Magdeburg, Central Germany (Firbas, Müller, and Münnich 1955), at Usselo, Province of Overijssel, Netherlands (De Vries, Barendsen, and Waterbolk 1958: 130-31; Van der Hammen 1951: chap. 4), and at Lago de Sanabria, Zamorra, Northwestern Spain (De Vries and Waterbolk 1958: 1551) indicate that this oscillation covers an interval of about 1,000 years between ca. 11,500 B.C. (GRO-705: 13,700 B.P. \pm 300 years and H-88/74: 13,250 B.P. \pm 280 years) and approximately 10,500 B.C. (H-77/54: 12,300 B.P. \pm 260 years; collected in the lower portion of Zone Ic). On this basis it is probable that the Older Dryas deposits of Zone Ic were accumulated during a period of not more than 500-years duration (compare Hipszeler 1957: 295). It is considered likely that during this relatively short cold oscillation the moraines of Central-Southern Sweden and those of the Leningrad region were formed. This event marks a short interruption in the Gotiglacial retreat, and it was immediately followed by the Allerød Oscillation. The latter, which corresponds with paleobotanical Zone II, started just before 10,000 B.C. and extended until about 8,850 B.C. (compare Flint and Deevey 1951: 266; Anderson, Levi, and Tauber 1953; Iversen 1953; Fromm 1953; Gross 1954, 1958; De Vries, Barendsen, and Waterbolk 1958; De Vries and Waterbolk 1958; Wright 1957). Certainly, as Gross (1954: 192) has pointed out, since this clearly-defined layer has been identified at so many localities in Northern and Western Europe, including the Auvergne region of Central France (cf. Dubois and Dubois 1944a, b, c) and Northern Spain (De Vries and Waterbolk 1958: 1551), it provides an excellent stratigraphic and chronologic key horizon, which, as shown by a large series of C-14 dates, lasted just over one millenium. After a temporary, but nonetheless rather severe, return of cold sub-arctic conditions between ca. 8,850 B.C. and approximately 8,000 B.C., when the ice-sheet stood at the Fennoscandian moraines (Younger Dryas: paleobotanical Zone III), Post-Glacial conditions were established throughout the region under consideration, except in the mountains and the high northern latitudes.

In concluding this very brief survey of Würm/Weichsel chronology, the hope is expressed that in the near future a more accurate figure for the end of the Göttweig Interstadial, as well as for the age of the basal portion of the Paudorf Oscillation, will be determined. Also it would be important to have a series of measurements on stratified geological samples dating from the late phases of the Middle Würm. However, notwithstanding the several gaps that exist, as well as the somewhat scattered nature of the data in certain instances from a geographical point of view, an absolute chronology covering a substantial portion of Fourth Glacial (Würm) time is beginning to emerge, as has been set forth in Figure 1. But the problem facing Palaeolithic archaeologists is this: exactly what cultural developments took place in Western Europe during this long interval of time? The answer to this question has a fundamental bearing not only on the time-span represented by the Upper Palaeolithic as a whole, but also on the actual rate of prehistoric culture change that took place in Europe during the critical period between approximately 60,000 and 10,000 B.C.

II. WESTERN EUROPE

GENERAL CONSIDERATIONS OF THE FOURTH GLACIAL SEQUENCE

Turning now to a consideration of the Fourth Glacial/Upper Palaeolithic sequence in Western Europe, it is fair to state at the outset that, on the basis of the field evidence, no Pleistocene geologist has yet been able to demonstrate the existence of a threefold subdivision of the Last (Würm/Weichsel) Glaciation within the actual glaciated area which can be correlated with a threefold subdivision of the Young Loess; and, strictly speaking, this is likewise true in the case of Central Europe. Indeed, from the geologic events registered in the Western Alps, Judson (1956: 67-69) has been unable to demonstrate the existence of more than one main Würm stage, although he strongly suspects that additional work in the region would prove that there was in fact an Early Würm (Würm I) advance, the deposits of which have been all but obliterated. This was followed by a Würm maximum (Middle Würm or Würm II + III), which produced the *moraines internes* and, after a retreat, there ensued a re-advance stage during which a series of moraines in the Geneva region were formed, which were assigned by Penck and Brückner to the Bühl re-advance. This Bühl re-advance has been placed by Woldstedt (1929: 313) and Gross (1954: 210) between the Zürich Stage (= Pomeranian) and the Gechnitz Stage (= Fennoscandian), and correlated with the Langeland Re-advance. Here the real difficulty is, as Wright (1957: 456-58) has recently emphasized, that one is beyond the range of effective paleobotanical evidence and dealing with formations that are as yet without C-14 dates, which means that any correlations of the Alpine sequence with the North European succession must be regarded as fluid. Only in the periglacial region of the north of France—i.e., the Seine Basin and the Somme Valley, areas which at no time were covered by ice during the Pleistocene—have stratified Fourth Glacial deposits of loess been described that are stated to be referable to the Würm I, II, and III Stages (compare Breuil and Koslowski 1931-1934; Breuil 1939; Bordes 1947, 1952, 1954, 1957; Bourdier 1956, 1957). But the correlations of these loess horizons with the Würm glacial cycle of the Alps are based on a series of assumptions which cannot be proved by direct geologic evidence, although it is apparent that much light could be thrown on this problem if adequate samples for C-14 dating were available for measurement.

In Northern France, as Bordes (1947, 1952, 1954, 1957) and other workers have clearly shown,⁵ the Young Loess (*loess récent*) is subdivided into three stratigraphic units on the basis of weathering horizons that occur between Young Loess I and II (a weakly-developed fossil soil) and between Young Loess II and III (a well-developed fossil soil). Thus, since there are

⁵ Actually the first investigator to recognize the threefold division of the Young Loess in Western Europe was Commont (1907: 349-51 and Fig. 43; also 1909a: 28-30 and Fig. 29; 1912a: 22), who over fifty years ago described in detail the zones of alteration on the surface of each of these horizons in the Somme Valley. Although the same three subdivisions also exist in Belgium (Tavernier and De Heinzelin 1957), in Southern Germany the basal unit in the series (Young Loess I) is lacking, as Bordes and Müller-Beck (1956, 1958) have recently shown.

three loesses, and since many authorities consider that there are three Würm Stages (compare Zeuner 1950, 1954, 1955,⁶ 1959: 110-32; Ebers 1955), some recent investigators have assumed that a direct correlation could be made on the basis of the simple equation: one loess = the maximum of one glacial episode. But the archaeological evidence suggests that there is something fundamentally wrong with this assumption, and this has been confirmed by the radiocarbon dates.

In the north of France and in Belgium, flint implements of Mousterian type are the only ones found in association with Young Loess I and Young Loess II. However, the only C-14 date which we have covering this interval is one that was determined by Rubin and Suess (1955: 486, Sample No. W-173) for a sample of peat collected in a lens just at the base of Young Loess II that was excavated from canal P.34 near Godarville in Belgium, 35 kilometers south of Brussels (compare Tavernier and De Heinzelin 1957: 308). The result—"older than 36,000 years"—suggests that the horizon in question presumably dates from an oscillation older than the Göttweig Interstadial. According to Bordes (*in litt.*, 16.1.58), typical Mousterian implements of Acheulian tradition: Levallois facies occur at this locality.

The earliest horizon in this region, which yields materials of very primitive Upper Palaeolithic type, is at Goderville, northeast of Le Havre, at the base of Young Loess III (Bordes 1954: 292-312). In the Somme region it appears that Commont (1908, 1909b, 1910, 1912b, 1912c, 1914, 1916) found two Upper Palaeolithic levels in the youngest loess, both of which yielded tools mainly of Upper Périgordian type, with the exception of a small series of typical Aurignacian artifacts, including carinate scrapers and several busked graters. The upper level is in the Post-Glacial weathering horizon near the top of Young Loess III (no Magdalenian material has ever been found in the Würm loess of Northern France), while the lower level consists of a thin band of pebbles in the middle portion of the same deposit. Presumably the Aurignacian series came either from the latter horizon or just below it. In the Paris Basin, Bordes (1954: 32-34, 129-31, 172-87, 282) found the same two levels: a few Upper Périgordian (Briquerterie de Duclair and Étrépagne) and Proto-Solutrean (Saint-Pierre-lès-Elbeuf) type artifacts from the altered zone near the top of the youngest loess, and a small Périgordian III series (Mons-Ivry, near Villejuif) in a pebble band that occurs near the mid-portion of Young Loess III, corresponding to the horizon reported by Commont in the Somme Valley. Bordes found no Aurignacian-type artifacts in association with this loess at any of the localities he investigated. This evidence conclusively demonstrates that no Upper Palaeolithic materials have ever been reported in Northern France in deposits older than Young Loess III. If the latter is definitely of Late Würm (i.e., Würm III) age, as claimed by many authorities, then one would be forced to admit that the entire Upper Palaeolithic development of Western Europe—Lower Périgordian, Aurignacian, Upper Périgordian, Solutrean, and Magdalenian—has taken place since the onset of the main phase of the Middle Würm, a view which is incompatible with the

data from Central Europe summarized in Part I of this paper. The latter evidence proves that in Hungary, Czechoslovakia, and Lower Austria the Upper Palaeolithic actually began one entire glacial stage earlier than many workers in Western Europe have been willing to admit. And this view is in very good agreement with the data recorded at the rich stratified occupation sites of the classic Dordogne region of Southwestern France. In this connection it should be pointed out that Valoch and Bordes (1957) have recently proposed a correlation of the Würm loesses of Czechoslovakia and Northern France, which may be summarized as follows: Young Loess I of Central Europe, often divided by a thin loam horizon into two units (? = the Loopstedt Interstadial; see page 358) that are designated Ia and Ib, corresponds to Young Loess I and Young Loess II of the north of France and Belgium. (This would, of course, agree with the Godarville evidence briefly discussed above.) On this basis, the thick and well-developed fossil soil horizon on the top of Young Loess II in Western Europe presumably was formed during the Göttweig Interstadial, suggesting that Young Loess III of the latter area corresponds to Young Loess II of Lower Austria and Czechoslovakia, and that the Central European Young Loess III has no equivalent in France and Belgium. It is also possible, and Bordes (1957: 573) agrees, that the Paudorf Oscillation is in fact represented in the latter region—albeit very feebly—by the thin band of pebbles (*cailloutis*) that separates the Young Loess III of the Paris Basin and the Somme Valley into an upper and a lower zone (compare Woldstedt 1956:80). This would place the Périgordian III of the pebble band at Mons-Ivry in the Paudorf Oscillation.

THE CLASSIC UPPER PALAEOLITHIC SUCCESSION IN SOUTHWESTERN FRANCE AND IMMEDIATELY ADJACENT AREAS

For establishing a tentative correlation between the Fourth Glacial climatic sequence and the Upper Palaeolithic cultural succession in Western Europe, the most important single archaeological site that has been investigated to date is Laugerie-Haute, Commune des Eyzies-de-Tayac (Dordogne), excavated by D. and E. Peyrony during the 1920's and 1930's (Peyrony and Peyrony 1938). Recently the Laugerie-Haute investigations have been resumed by Bordes and Mme. de Sonneville-Bordes (Bordes 1958b; 1959: 160-61). The stratigraphic succession in the West Sector of this large and very impressive site, which differs somewhat from that of the East Sector, is as follows:

- Neolithic and Later.
- Azilian (East Sector only).
- I Magdalenian I, II, and III occupation layers (I', I'', and I''') overlain by huge blocks fallen from the roof of the shelter.
- H Three Solutrean levels (H', H'', and H''') separated by sterile layers (H).
- G Lower Solutrean.
- E Sterile layer composed of small limestone conglifrats (*éboulis*) lying in a matrix of sandy soil. 40 centimeters to 1.00 meter.

⁶ See Bordes 1958a, for an excellent critique of Zeuner 1955.

- D Brown archaeological horizon containing Aurignacian V hearths. 40 centimeters.

[In the East Sector of the site this level (Bordes' *Couche* 33) has recently been identified by Bordes and De Sonneville-Bordes (De Sonneville-Bordes and Bordes 1958); it immediately overlies Peyrony's *Couche* F (Proto-Magdalenian), which in turn corresponds to Bordes' *Couche* 36 (compare Bordes 1958b: 212; 1959: 160-61).]

- C Sterile layers composed of small, angular limestone elements (conglifraacts) lying in a matrix of dark brown to reddish sandy soil, which separate: } 1.20 meters

- B Three thin lenses of Périgordian VI hearths.

[Actually, this level was originally assigned to the *Périgordian III* by Peyrony (1933: 549-52; with E. Peyrony, 1938: 12-21), mainly on the basis of the typological evidence. Recently the term "*Périgordian VI*" has been proposed as the result of investigations at the Abri Pataud (Movius 1960a, 1960b; with Vallois, 1960: 221; Bordes 1959: 158-61; De Sonneville-Bordes 1960: 177-78, 226), where the same assemblage has been found overlying a rich *Périgordian Vc* (Noailles Burin) horizon and, in turn, overlain by the Proto-Magdalenian of Peyrony's *Couche* F at Laugerie-Haute: Est. It also occurs in the East Sector at Laugerie-Haute (Bordes 1958b: 236-40, 1959: 160-61; De Sonneville-Bordes 1958: 438-42, 1960: 168-72), in association with an *éboulis* formation apparently accumulated during the same prolonged cold interval as that which prevailed during the overlying Proto-Magdalenian, Aurignacian V, and Lower Solutrean occupations.]

- A Small, angular, loosely-lying, limestone conglifraacts with air-spaces between them (*éboulis secs*), resting on the bedrock floor of the shelter. 60 centimeters to 1.00 meter.

The basal level (*Couche* A) of this rock shelter, which is composed of what French glacial geologists call *éboulis secs*,⁷ rests directly on the bedrock floor of the site, which slopes outward toward the Vézère River, a tributary of the Dordogne. Although this floor is 4.50 meters above the rocky bed of the Vézère, nowhere in the Laugerie-Haute deposits were there detected any traces of flooding or of water-laid deposits of any sort. Indeed, the composition of the conglifraact horizon very definitely suggests accumulation during an interval of intense cold, but the question is with which Late Glacial climatic event should these *éboulis* be correlated? However, unfortunately this horizon (*Couche* A) at Laugerie-Haute, which is overlain by a series of Périgordian VI hearths, is sterile archaeologically; very probably it was accumulated during the main phase of the Middle Würm.

As to the climate of the Mousterian, Peyrony (1939: 58-59) has observed that initially cold and relatively

⁷ Peyrony (1939: 54-55) and many other workers regard a formation of *éboulis secs* in limestone country as indicative of a cold, dry climate of continental type at the time of deposition. It is believed that under such conditions there would have been relatively little humidity, and consequently only the superficial zone of the limestone would have absorbed moisture. On freezing this would expand and as a result the outermost layer of the rock would be detached in small fragments. Thus, despite very pronounced cold conditions, only thin angular scales, or conglifraacts, of limestone would accumulate in the adjacent deposits. On the other hand, if the climate had been humid and cold, the limestone (which is often fissured to a considerable depth) would absorb a great deal of moisture. Under freezing conditions this would result in the collapse of huge blocks from the ceiling and roof of a given cave or rock shelter; in fact, often the complete overhang of the latter would fall.

damp conditions which in turn gave way to a cold, dry climate obtained throughout Western Europe. This is borne out not only by the nature of the sediments, but also by the evidence of the fauna. On the other hand, conditions during the earliest stage of the Upper Palaeolithic—the Périgordian I (or Châtelperronian)—were far less rigorous. At several sites this cultural horizon occurs in deposits that are of reddish color, and the contained faunal remains are very badly preserved. Although Reindeer was abundant, bones of Red Deer (*Cervus elaphus*) are more numerous in this level than in the underlying Mousterian strata. These observations are in accord with the suggestion that, at least in Southwestern France, the then-existing climate was somewhat more temperate and moister than formerly. Such a view is consistent with Peyrony's suggestion (1938: 76) that the Périgordian I development took place during the time interval of the Würm I/II, or Göttweig, Interstadial. One postulates that during this interval the melting of great masses of snow and ice in the Vézère drainage, which had accumulated during the preceding episode of glacial advance, led to a marked rise in the level of the river. As shown in Part I of this paper, the Göttweig Interstadial roughly covers the span between ca. 42,000 and approximately 30,000 years ago. But as yet it is unknown at what horizon during this long interval of relatively unstable climate, when conditions varied between sub-arctic and boreal, the earliest Upper Palaeolithic began to develop in Western Europe. Thus far the following C-14 dates are the only ones available for an archaeological deposit covering this range of time in a region immediately adjacent to the one under consideration:

GROTTE DU RENNEGRO-1736: 33,500 ± 400 yrs. (31,550 B.C.)
GRO-1742: 33,640 ± 250 yrs. (31,690 B.C.)

Grotte du Renne, Arcy-sur-Cure (Yonne). Charcoal and ash from hearth in Level VIII, which yields a developed Périgordian I (Châtelperronian) industry (Leroi-Gourhan 1949, 1952; Bailloud 1953). These figures are unpublished: the writer is grateful to Mme. Leroi-Gourhan and H. de Waard for the above information.

Comment. These figures provide us for the first time with an accurate date for an early Upper Palaeolithic occupation in France. Previous estimates quoted in the literature vary anywhere from 70,000 to 25,000 years ago, but until recently this was all we had on which to base an absolute chronology.

In the horizon overlying the Périgordian I, evidence of intense periglacial conditions is once more apparent. As Peyrony (1939: 56; 1947: 183) has stated, at many of the Dordogne stations (La Gravette, Cro-Magnon, Abri Lartet, Abri du Poisson, Abri Cellier, Abri Pagès, Abri Blanchard, Abri Castanet, etc.; cf. Peyrony 1949, for references) the Aurignacian I (with cleft-base bone points) is contained in beds composed of limestone *éboulis*, or conglifraacts, which are very similar in every respect to those of the basal level at Laugerie-Haute. Furthermore, at these Aurignacian I sites one normally finds a high arctic fauna that is absent in the older levels (compare Bouchud 1952b: 268; 1959). This includes such forms as: the Greenland Seal, the Lemming, the Arctic or Blue Fox (also called the Isatis; see Bouchud

1951), the Musk-Ox, the Suslik (*Spermophile*), and a great abundance of Reindeer. On the basis of these observations, it seems logical to correlate the beginning of the typical Aurignacian cultural development in the Dordogne region with the onset of the somewhat more rigorous climatic conditions synchronous with the early phase of the Middle Würm, or Würm II, Glacial Stage. Recently C-14 dates have been obtained for samples from three Aurignacian horizons in France. Since none of these has been published, the writer has the excavators and H. de Waard of Groningen to thank in each case for the following information:

LA QUINAGRO-1493: 31,170 \pm 350 yrs. (29,220 B.C.)
GRO-1489: 30,760 \pm 490 yrs. (28,810 B.C.)

La Quina, Commune de Gardes (Charente). Charcoal and ash from large hearth in the Aurignacian I occupation layer at this extensive rock shelter. The faunal remains indicate that sub-arctic climatic conditions prevailed at the time of occupation (Henri Martin 1925a, 1925b, 1930, 1936; Alimen, G. Henri-Martin, and Guillien 1946; G. Henri-Martin 1956; Patte 1954, 1956).

Comment. This figure is approximately 1,000 years older than the estimated age of the Aurignacian I in France based on the geological and palaeontological data.

ABRI CAMINADEGRO-1491: 28,880 \pm 250 yrs. (26,930 B.C.)
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Abri Caminade, Commune de La Canéda, near Sarlat (Dordogne). Charcoal and ash from Aurignacian I hearth (De Sonnevile-Bordes and Mortureux 1955; Mortureux and De Sonnevile-Bordes 1956; De Sonnevile-Bordes 1955a, b).

Comment. According to Bordes (*in litt.*, 19.2.60), the Aurignacian I of this site is typologically evolved. Furthermore, the sample was small and only contained sufficient carbon for a single measurement.

Grotte du RenneGRO-1717: 30,370 \pm 300 yrs. (28,420 B.C.)
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Grotte du Renne, Arcy-sur-Cure (Yonne). Charcoal and ash from hearth in Level VII (Typical Aurignacian). See above for references.

Comment. This figure fits exceedingly well with the estimated age of the early Aurignacian. In her paper, which is to be published in the *Comptes Rendus du Congrès Préhistorique, Monaco, 1959*, Mme. Leroi-Gourhan has placed the Aurignacian horizon (Level VII) at the Grotte du Renne in her "Interstade d'Arcy," and in a letter dated Paris, 8 November 1959, she states that pollen of hazel, oak, alder, linden, ash, and ferns—definitely indicative of a mild climate—have been identified in the Level VII deposits. On this basis, it is very likely that this occupation occurred during a minor oscillation following the intense cold interval registered at so many Aurignacian I sites in the Dordogne and Charente regions. In any case, this is not regarded as an interval of interstadial magnitude.

An earlier series of charred bone samples from this site was collected during the 1955 field season and measured at the Lamont laboratory (Olson and Broecker 1959: 23). These gave the following results:

L-340A: Level V	(Upper Périgordian) . . 11,400 \pm 250 yrs.
L-340B: Level VII	(Typical Aurignacian)
10,900 \pm 250 yrs.
L-340C: Level IX	(Lower Périgordian) . . 15,700 \pm 400 yrs.

L-340D: Level X (Lower Périgordian) . . 15,350 \pm 400 yrs.

The amount of material available in these samples was insufficient to give them normal pre-treatment. Therefore the figures listed can only be considered as providing minimum dates for the four horizons in question.

In connection with the Arcy evidence, it should be noted that the Aurignacian II development in the Dordogne occurred during an interval of climatic amelioration. As Bouchud (1952b: 269; 1959) has shown, at the Abri Castanet, Commune de Sergeac (Dordogne), the evidence of the rodents and birds indicates that conditions became somewhat milder and that there was an extension of the forests during Aurignacian II times. Here one finds the paradoxical fact of the presence of the Garden Dormouse (*Eliomys quercinus*) and the Banded Lemming (*Lemmus lemmus*) together with an avifauna characteristic of a temperate climate. Presumably this relatively short climatic oscillation, which has not been reported at any of the Central European loess stations, corresponds to the one registered at the Grotte du Renne (Arcy-sur-Cure), referred to above. Also at La Quina, it has been demonstrated by Alimen, G. Henri-Martin, and Guillien (1946: 115) that the deposits overlying the Aurignacian I horizon were accumulated under the conditions of a fairly mild humid climate.

During the ensuing Aurignacian III/IV and Upper Périgordian, beds containing a high component of limestone *éboulis* still continued to be formed, but these horizons normally contain some soil brought in by small run-off streams flowing down the slopes adjacent to the sites. For the early Upper Périgordian—Stage IV in D. Peyrony's sequence—we have a good C-14 date of ca. 22,000 years B.C. for a large hearth, five to seven centimeters thick, that was excavated in 1953 at the base of a very rich and typical Périgordian IV (Gravettian) occupation layer in Trench II, during the course of a small test excavation at the Abri Pataud, Les Eyzies (Dordogne). The actual figures obtained for the Abri Pataud sample by Rubin and Suess (1955: 487; Movius 1955: 39) are as follows:

W-151:.....	23,600 \pm 800 yrs. (21,650 B.C. \pm 800 yrs.)
W-152:.....	24,000 \pm 1,000 yrs. (22,050 B.C. \pm 1,000 yrs.)

This means that the occupation in question not only belongs in the early portion of the main phase of the Middle Würm, but also that the Périgordian IV (Gravettian) development in Aquitania is very slightly younger than the assemblages from Pavlov and other similar localities in Moravia (see page 390). These Central European stations were occupied during the time interval when the lower portion of the Young Loess III formation was being accumulated (i.e., immediately after the end of the Paudorf Oscillation), which actually agrees very well with the stratigraphic evidence from the Paris Basin and the Somme Valley, referred to on pages 364-65. This interpretation is in accord with the geological and paleontological evidence of the Dordogne archaeological sites. But there is one serious difficulty, namely, the Paudorf interval itself does not seem to be clearly registered either at La Ferrassie (see

below) or at any of the other Dordogne localities excavated to date, with the possible exception of the Abri Pataud (Movius 1954, 1955). Here a clearly-defined "sterile" sand layer (Deposit G of Trench I) intervenes between the underlying Aurignacian I/II of Deposit H and the very rich Périgordian IV of Deposits F and E immediately above, both characterized by an abundance of *éboulis* and a "cold" fauna. As to the overlying Périgordian Vc and VI horizons, the same observation applies; however, a preliminary study of the fauna suggests that, although a sub-arctic climate prevailed, conditions were somewhat less rigorous than was the case during the ensuing Proto-Magdalenian occupation (Movius 1960a, 1960b; with Vallois, 1960: 218). At the nearby site of Laugerie-Haute, the entire sequence of deposits from the base of the section up to the Lower Solutrean, containing the Périgordian VI, Proto-Magdalenian, and Aurignacian V horizons, consists of thermoclastic elements accumulated during a single cold interval. As Bordes (1958b: 243) has clearly stated, there is not the slightest indication here of a formation referable to the Paudorf Oscillation, which is defined in the north of France by the *cailloutis* bed that separates the Recent Loess III into two sub-units and which, as pointed out on page 365, has produced an assemblage of Périgordian III type. Admittedly this problem is in urgent need of further detailed investigation, and, as the work progresses at the Abri Pataud, it is hoped that pertinent data will be forthcoming.

At the large rock shelter of La Ferrassie, Commune de Savignac-de-Miremont (Dordogne), a sequence of climatic events very similar to that outlined above has been registered (Peyrony 1934). The Mousterian levels containing Neanderthal burials are composed of *éboulis* and associated with a "cold" fauna, whereas in the overlying Périgordian I and "Aurignacian 0"⁸ horizons (Deposits E and E') relatively few *éboulis* occur, remains of Deer and other forest forms are more frequent, the bones are badly decomposed and heavily weathered, and the characteristically reddish clayey earth with very small limestone elements contains a high component of slope-wash material. The overlying levels have furnished the first four phases of the Aurignacian industry, followed by a series of thin Périgordian V (a, b, and c) occupations. In addition to a cold fauna, these beds yield a large quantity of *éboulis* of small- to medium-size limestone components, including some rather large blocks detached from the vault of the shelter by frost-action. Finally, the shelter is completely filled by an extensive talus formation, the lower part of which is composed of earth plus very small limestone elements, apparently accumulated during a somewhat less cold interval. Then, in the upper part, there re-appear the typical limestone *éboulis*, which seem to correspond to a new lowering of the temperature.

This latter interval of severe cold also occurs at approximately the same stratigraphic horizon at both Laugerie-Haute: Est (Bordes 1958b: 243) and the Abri

Pataud, at each of which localities the Proto-Magdalenian level consists of a thermoclastic formation of *éboulis* overlying the Périgordian VI horizon (see page 365 for the Laugerie-Haute sequence). In each case a cold fauna of sub-arctic type has been identified, according to Bouchud (1959; Movius 1960a, 1960b; Movius and Vallois 1960: 215). Various samples from this level (*Couche* 2) at the Abri Pataud were submitted to the late Professor De Vries for C-14 measurement, and the results (unpublished) are as follows:

ABRI PATAUD—Proto-Magdalenian Level: 1958 Measurements

GRO-1867: Ash without humus extracted . . .	18,970 ± 240 yrs. (17,020 B.C.)
GRO-1853: Ash with humus extracted . . .	20,600 ± 280 yrs. (18,650 B.C.)
GRO-1857: Charred bone	20,720 ± 220 yrs. (18,770 B.C.)
GRO-1862: Uncharred bone	21,700 ± 250 yrs. (19,750 B.C.)

In order to check the above results, additional samples of solid uncharred bone were sent to De Vries on 22 July, 1959. During the period 5 October–11 December he conducted *very extensive* experiments on the chemical pre-treatment of these solid bone samples, and, from the trend of the results, it appears that he gradually managed to extract the oldest carbon. According to H. de Waard of the Groningen laboratory, to whom the present writer is very grateful for this information, of a total of sixteen age determinations carried out on different chemical fractions of these samples, there is a very consistent series:

GRO-2069:	20,230 ± 200 yrs. (18,280 B.C.)
GRO-2071:	20,000 ± 200 yrs. (18,050 B.C.)
GRO-2081: Duplicate of GRO-2071	20,600 ± 200 yrs. (18,650 B.C.)
GRO-2100: Collagenous proteins	20,000 ± 200 yrs. (18,050 B.C.)
GRO-2115: Collagenous proteins soluble in alkali	20,200 ± 200 yrs. (18,250 B.C.)

On the basis of the above figures, it would appear that *ca.* 18,250 B.C. would be a fairly reliable date for the Proto-Magdalenian occupation at this locality. Unfortunately, no reliable measurements are as yet available for the underlying Périgordian VI horizon.

At Laugerie-Haute: Est, where the Proto-Magdalenian level was originally recognized over thirty years ago by D. Peyrony (1929, 1935), the stratigraphic relationship between the Aurignacian V of the West Sector and the Proto-Magdalenian of the East Sector has recently been established (De Sonneville-Bordes and Bordes 1958; Bordes 1959: 160–61); as stated on page 365, the Aurignacian V immediately follows the Proto-Magdalenian and, in turn, it is directly overlain by the Lower Solutrean. The results of De Vries' measurements (unpublished) for samples collected by Bordes and Mme. Bordes in the Proto-Magdalenian (Peyrony's *Couche* F; Bordes' *Couche* 36) and the Lower Solutrean

⁸ Formerly considered "Périgordian II," but, as De Sonneville-Bordes (1955a, b) has conclusively shown by her detailed statistical examination of the assemblages in question (as well as the assemblages from other allegedly "Périgordian II" sites in France), this horizon must henceforth be considered as Aurignacian and eliminated from the Périgordian group.

(Peyrony's *Couche G*; Bordes' *Couche 31*) horizons at Laugerie-Haute: Est are as follows:

- GRO-1888: Lower Solutrean level 20,650 \pm 300 yrs.
(18,700 B.C.)
GRO-1876: Proto-Magdalenian level 21,735 \pm 250 yrs.
(19,785 B.C.)

Whereas these results are internally consistent, the figure for the Proto-Magdalenian (GRO-1876) appears to be about 1,500 years too old in terms of the recent Abri Pataud dates. Doubtless this discrepancy will be straightened out when new samples from this, as well as from the overlying Aurignacian V and Solutrean horizons, have been processed and measured. In the meantime, it should be noted that the Proto-Magdalenian figure (GRO-1876) is almost identical with one of the original 1958 dates (GRO-1862) for the corresponding horizon at the Abri Pataud.

In Eastern France, samples collected at a site belonging to this same range of time on the basis of the archaeological evidence—the large rock shelter of La Colombière, near Poncin (Ain)—have been dated by the C-14 method. However, as pointed out elsewhere (Movius and Judson 1956: 35, 144–45 and 150), the figure of ca. 12,200 B.C. (L-177: 14,150 B.P. \pm 450 years) obtained by the Lamont laboratory for ashy material found in a demonstrably Final Périgordian hearth (? Périgordian VI) at La Colombière (Broecker and Kulp 1957: 1329–30) is certainly too low. Since a check run (W-150) at the Washington laboratory produced an even younger C-14 date (11,750 B.P. \pm 600 years) for ash from the same hearth (Rubin and Suess 1955: 487), it is concluded that field contamination was present in the vicinity of this feature. As stated on page 373, the geological evidence shows that the Final Périgordian occupation at this site occurred just as the main Middle Würm ice retreat was getting under way.

At Laugerie-Haute the geologic implications of the deposits constituting the upper portion of the section suggest a gradual improvement in the climate. The Lower Solutrean is included in thermoclastic deposits, and Bordes (1958b: 244) points out that the upper part of the level seems to have been subjected to cryoturbation activity which has rounded the limestone elements. In the Middle and Upper Solutrean, however, as well as in the Lower Magdalenian (I–II) horizons (Peyrony's Deposits H and I), the sediments become much more sandy and even clayey in the upper part, and the small-to medium-size angular limestone fragments, typical of thermoclastic *éboulis* formations, and large blocks fallen from the roof, are far less common. According to Peyrony (1939: 55), these deposits contain much more stream-transported earth and aeolian-derived materials than are present in the underlying levels. Here, as elsewhere in Southwestern France, the Lower Magdalenian definitely seems to have developed during a relatively temperate interval (compare Bordes 1958a: 166), which had already begun before the close of the Solutrean.

In the Magdalenian III layer, the thermoclastic *éboulis* begin to occur again, but are not numerous (Bordes 1958b: 244), and they only re-appear in high frequency in the uppermost part of the Magdalenian V level at this locality (*Couche K*; cf. Peyrony 1939: 56) and at the adjacent site of Laugerie-Basse. Therefore,

it seems reasonably clear that during the Middle and Upper Solutrean and the first two phases of the Magdalenian, the climate—although cold—was somewhat less sub-arctic in character than it had been previously. Indeed it seems very likely that during the major portion of the time-span covered by the Solutrean stations in the Dordogne, cold boreal conditions prevailed characteristic of a relatively dry climate of continental type with fairly short summer seasons separated by long winters. Such conditions are clearly indicated by the evidence of the fauna recovered from these Solutrean sites, which represents a typical Late-Glacial assemblage: remains of Reindeer occur in highest frequency, followed by Horse. Bones of Mammoth, Woolly Rhinoceros, Wild Ox, Bison, Ibex, Chamois, Stag, Wild Boar, etc., are also reported in small quantities. On the other hand, a detailed study of the faunal materials (especially the rodents and birds) from the Abri Lachaud, near Terrasson (Dordogne), supports the view that a slight amelioration of the climate, accompanied by an extension of the forests, was already under way during Upper Solutrean times and that this situation prevailed until the end of the Magdalenian II occupation (Bouchud 1952a). Although not a single locality in France referable either to the Middle-Upper Solutrean or to the Lower Magdalenian ranges of time has ever been dated by the C-14 method, it is tentatively suggested that this relatively mild interval represents a climatic response to the retreat of the North European ice-sheet from the Frankfurt to the Pomeranian moraines—a view which, within certain limits at least, is in good general agreement with the radiocarbon evidence for the age of the Middle Magdalenian. The data bearing on this subject, based on measurements of samples from four sites—two in France and two in Spain—are given below.

LA GARENNE

La Garenne, near Saint-Marcel (Indre). In 1951, Libby (1951: 291–92; 1955: 85–86) ran a series of measurements on samples collected by Allain (1953: 293–94) in and around a large hearth that was buried under a huge fall of rock consisting of blocks up to 2.00 meters in diameter. The hearth was approximately 60 centimeters long and 10 centimeters thick at the center. The results are as follows:

- C-577: 1500 grams of burned bone from which sufficient organic material was obtained by acid dissolution of the bone 11,109 \pm 480 yrs.
(9,159 B.C.)
C-578: Same, except that it consisted of ashy material mixed with sand, charcoal, and burned bones 15,847 \pm 1,200 yrs.
(13,897 B.C.)
C-579: Same as C-577 and C-578, except that the material was found outside the hearth but in the same horizon. The material consisted of burned bones 12,986 \pm 560 yrs.
(11,036 B.C.)

Comment. It can be seen that the figures for burned bone samples are lower than in the case of the ashy material mixed with charcoal. But one cannot accept the fairly plausible dates of 13,897 B.C. (\pm 1,200 years) and 11,036 B.C. (\pm 560 years) for this hearth and reject the other figure. This is

an excellent illustration of the kind of problem with which the archaeologist is constantly faced—inconclusive results from one set of samples, while it may take him several years of excavating to bring to light suitable new material for use in connection with running check measurements.

In an effort to solve the problem of the age of the Middle Magdalenian occupation at La Garenne, a large sample of burned bone was collected several years later by Allain (1957; with Descouts, 1957) in a new hearth (*Foyer II*) found in an extension of the cave that is separated from the main part by a rock sill. The conditions for preserving perishable materials were believed to be much better here than in the main cave, where, as stated above, the occupation layer is overlain by a thick rock-fall. The result has been published by Olson and Broecker (1959: 23) and is anything but satisfactory; it is given below:

L-399D: Residue after removal of humic acid and bone carbonate 9,500 \pm 500 yrs.
(7,550 B.C.)

Comment. Sample portions treated in several ways for dating—bone carbonate: 9,580 \pm 200 years, and untreated sample of burned bone: 9,150 \pm 230 years—are in good agreement with the above figure. In this connection, one should point out that the original date announced for the residue fraction of sample L-399D, which was 14,200 B.P. \pm 500 years (ca. 12,200 B.C.), falls squarely in the middle of the range of at least two of Libby's figures (C-578 and C-579). However, after a re-run on this same sample component, Olson and Broecker (1959: 28, note) decided to change the date. If the true age of L-399D proves to be 9,500 B.P. \pm 500 years, then this hearth must represent an Early Mesolithic intrusion into this otherwise Middle Magdalenian occupation layer, a possibility which is considered to be very unlikely.

ABRI DU ROC-AUX-SORCIERS. GRO-1903: 13,920 \pm 80 yrs.
(11,970 B.C.)

Abri du Roc-aux-Sorciers, Angles-sur-l'Anglin (Vienne). Sample of charcoal and ash from Middle Magdalenian occupation layer (Garrod and De Saint-Mathurin 1949, 1950; De Saint-Mathurin and Garrod 1951).

Comment. The writer is grateful to H. de Waard of Groningen for the above figure, which is unpublished and which falls at the extreme upper limit of the range for the Middle Magdalenian as nearly as can be determined on the basis of the available data.

The C-14 laboratory at the University of Michigan has recently run measurements of Magdalenian III samples from two sites, both near Santander, in Northern Spain. The writer wishes to thank James B. Griffin for the information (*in litt.*, 20.11.59), which is summarized below:

ALTAMIRA. M-829: 15,500 \pm 700 yrs.
(13,540 B.C.)

Altamira, Santillana del Mar, 30 kilometers west of Santander, Northern Spain (Breuil and Obermaier 1935: 157-98). Sample collected by J.B. Griffin of the University of Michigan (U.S.A.) and J. González Echegaray of Santander, in the Magdalenian III midden deposits on the left inside the entrance of this famous cave.

CUEVA DEL JUJO. M-830: 15,300 \pm 700 yrs.
(13,340 B.C.)

Cueva del Juyo, 7 kilometers from Santander and 4 kilometers from the coast of Northern Spain. According to the excavators, Janssens and González Echegaray (1958: 97), this site may be attributed to the typical Magdalenian III of the Cantabrian coastal region.

Comment. As a result of these measurements—i.e., Altamira and El Juyo—we may conclude that approximately 13,400 B.C. is fairly close to the date for the Magdalenian III development in Northern Spain. The above figures will be published shortly (1960) by Crane and Griffin.

The climatic conditions which prevailed in the Dordogne region of Southwestern France during Magdalenian IV, V, and VI times are clearly reflected in the stratigraphic successions of the large La Madeleine rock shelter (Capitan and Peyrony 1928: 15-19; Peyrony 1939: 56-57) and the nearby Abri Villepin (Peyrony 1936), both in the Commune de Tursac (Dordogne). Indeed, as De Sonneville-Bordes (1956: 377) has recently observed, these two sites provide one of the best sequences covering the terminal portion of the Upper Palaeolithic which exists anywhere in Western Europe. At La Madeleine, which is at an elevation of only a few meters above the level of the present floodplain of the Vézère River, the horizon with the primitive types of barbed bone points, and the lower division of the one which yields bone points with a single row of barbs (Magdalenian IV and Va; compare Breuil 1937: 44-56, 1954b: 61; Breuil and De Saint-Périer 1927: 3-5; Breuil and Lantier 1959: 185-89), consist mainly of sands laid down by stream action (*sables d'inondation*), indicating that the site was frequently subjected to flooding by the waters of the Vézère during the time of these occupations. These same floods practically filled with water-laid sands a nearby Upper Magdalenian and Azilian rock-shelter site, the Bout-du-Monde, in the Commune des Eyzies and just downstream from La Madeleine (Peyrony 1947: 182-83). Presumably these inundations were caused by the melting of snow and ice in the adjacent highlands during an interval of temperate conditions. The fauna in this horizon at La Madeleine consists of a predominance of Reindeer, together with a rather high frequency of Horse. On the other hand, in the upper portion of the level with unilaterally barbed bone points and the first half of that with bone points with two rows of barbs (Breuil's Magdalenian Vb and VIa), the deposits are composed mainly of loosely-compacted, small *éboulis secs*, with no trace whatsoever of water-laid sand. The fauna includes an abundance of Reindeer, while Horse remains are rarer here than in the underlying horizon, and they are associated with bones of Chamois, the Pika or Tailless Hare (*Lagomys*), and the Snow Partridge, indicating that intensively cold conditions had set in. Now it was that the maximum distribution of the Reindeer was attained, great herds having roamed for the first time south of the Pyrenees into the Cantabrian region of Spain, and even to the Mediterranean coast near Narbonne.⁹ Other sites in Western Europe have furnished comparable data, and therefore it is assumed that this final return of sub-arctic conditions in the Dordogne region should be synchronized with the temporary halt of the retreating North European ice-sheets at the Langeland-Samland

⁹ On the basis of the data from the Belgian localities, Tavernier and De Heinzelin (1957: 307-308) have recently stated that the last cycle of intense cold occurred during Middle Magdalenian times. Furthermore, they suggest that the Upper Magdalenian is more or less contemporary with the beginning of the Allerød Oscillation, a view which is somewhat at variance with the data from the Southwestern French localities discussed in this paper.

moraines when the Oldest Dryas solifluction levels of paleobotanical Zone Ia were accumulated on the North European Plain. It is believed that this interval had come to a close by approximately 11,500 B.C., which marks the beginning of the Bölling Oscillation (paleobotanical Zone Ib). As stated on page 363, this latter event occurred in the early portion of the Gotiglacial Retreat in Fennoscandia. Such a correlation would in fact be in accord with the implications of the La Madeleine and Abri Villepin stratigraphy. At the former site, in the uppermost level with bilaterally barbed bone points of Magdalenian VIb type, the fauna becomes considerably more temperate: the Reindeer and the Horse are less abundant, while such forest forms as the Red Deer and the Wild Boar appear.

This evidence for the setting in of temperate conditions at the very end of the Magdalenian is further attested at the Abri Villepin, where the Magdalenian VIa occurs in a water-laid deposit of clayey sand and is associated with an abundance of Reindeer. In the overlying Magdalenian VIb level the frequency of Reindeer diminishes, while that of Red Deer increases. Finally, just above a thin flood-water deposit that is archaeologically sterile, there is an Azilian horizon in which the Reindeer has practically disappeared, the frequency of Horse has considerably diminished, and the Red Deer shows a marked increase.

This sequence, established by Peyrony for the Périgord, is very similar in all essential respects to what Guillian (1943, 1944, 1947) has demonstrated for the Charente. Following an episode characterized by the accumulation of huge blocks fallen from the vaults (possibly indicative of movements of seismic origin) at various sites in the Dordogne and the Gironde (Laurerie-Haute, Jean-Blanc, Saint-Cirq, Les Grands Rochers, Saint-Germain-la-Rivière) at the very end of Magdalenian III times,¹⁰ the climate became temporarily milder during the Magdalenian IV and Va Stages. These conditions in turn gave way to the onset of intense cold during the Magdalenian Vb and VIa intervals, and, as Bordes (1958a: 166) has observed, it was not until during Magdalenian VIb and continuing into the Azilian that one notes the beginning of a temperate climate with the establishment in Southwestern France of definitely Post-Glacial conditions. On this basis, it seems quite likely that the Magdalenian VIb and part at least of the Early Azilian developments took place during the time of the Bölling Oscillation and accordingly date from the middle of the twelfth to the middle of the eleventh millennia B.C. Thus far one site only in Western Europe covering this range of time has been dated by the radiocarbon method; the salient facts are summarized below.

GROTTE DE LA VACHE

Grotte de la Vache, near Tarascon-sur-Ariège, Commune d'Alliat (Ariège). Samples of charcoal and ash collected by Romain Robert from several different horizons in the deposits have been measured by the Lamont and Groningen laboratories. Only preliminary reports on this important site have thus far been published, which include papers by Robert (1951, 1953), Malvesin-Fabre, Nougier, and Robert

(1951), Breuil and Robert (1951), and Nougier and Robert (1956). The following three dates have been determined:

L-336C:	Charcoal and ash from <i>Couche</i> 2: 10-centimeter to 20-centimeter horizon (Broecker and Kulp 1957: 1329)11,650 \pm 200 yrs. (9,700 B.C.)
GRO-2025:	Charcoal and ash from <i>Couche</i> 212,540 \pm 105 yrs. (10,590 B.C.)
GRO-2026:	Charcoal and ash from <i>Couche</i> 412,850 \pm 60 yrs. (10,900 B.C.)

Comment. The writer is grateful to Robert for the information (*in litt.*, 23.2.60) on the results of the two Groningen measurements. In comparison with these, the Lamont date appears to be approximately 1,000 years too young.

Before terminating this paper, one additional radiocarbon-dated Upper Palaeolithic locality in France—the famous Grotte de Lascaux, near Montignac (Dordogne)—should be considered. This cave, one of the most important single Upper Palaeolithic art sites ever discovered, was found in 1940 (cf. Movius 1951; also Laming 1959, for additional information). The Lascaux sample, which has the distinction of having been the first one from a Palaeolithic site in the Old World to be submitted for C-14 dating, was recovered in 1949 by the Abbé H. Breuil and Séverin Blanc. It consisted of charcoal that has been identified by Barghoorn (with Movius, 1951) as *Abies*, probably the European Silver Fir (*A. pectinata*), and was collected during the course of excavations in the pit, or lower fissure, in the north-western portion of the cave. The material was associated with some very badly preserved bones of Red Deer, a few nondescript flint implements, one long javelin point of Deer antler and a large number of medium-size limestone slabs that had been collected during prehistoric times on the surface of the adjacent plateau and brought into the cave. These slabs are more or less flat, and they had been carefully selected because of their naturally concave upper surfaces. These exhibit traces of carbon in each case; apparently they were originally brought to the cave for use as crude lamps. The date, announced in 1950 by Libby, then of the University of Chicago (Arnold and Libby 1951: 112; Libby 1955: 85) is as follows:

C-406:	Charcoal from the Grotte de Lascaux15,516 \pm 900 yrs. (13,566 B.C.)
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Comment. Many authorities believe that from a stylistic point of view the majority of the Lascaux paintings are Upper Périgordian in date—Phase 2 in the Upper Palaeolithic art sequence of Western Europe—while others maintain that many of the works of art in question are of Middle Magdalenian age (compare Blanc 1953; Breuil 1954a), as discussed in detail by Mlle. Laming in Chapter 2 of her recent book (Laming 1959). However this may be, the above date is perhaps as much as 5,500 years too young for the Upper Périgordian, while, since it is in good agreement with the age of the Middle Magdalenian, probably should be accepted as proof that the cave was still in use at that time. Indeed, the possibility that this charcoal is of Magdalenian age accords with the evidence of the paintings in

¹⁰ It is possible that this interval of increased thermoclastic activity (see page 369) should be correlated with the temporary halt of the ice at the Pomeranian moraines of Northern Germany.

the pit, or lower fissure, near which it was collected. But even evidence of this nature is at best indirect and cannot be accepted as providing a conclusive solution to the problem of the age of the paintings themselves. In this connection, one would be in a far stronger position if an engraved art object exhibiting either the style of the Upper Périgordian, on the one hand, or of the Middle Magdalenian, on the other, should be found in the lamp-and-charcoal-bearing horizon. Otherwise it is difficult to understand how Libby's date can ever be correlated even indirectly with any single group of the magnificent and very important Lascaux murals.

Originally it was believed that the entrance to the Grotte de Lascaux had been completely sealed off from the outside world since late Fourth Glacial times, but this assumption was not confirmed by the C-14 dates announced several years ago by De Vries and Waterbolk (1958: 1553), which indicate that the site was still inhabited as late as Mesolithic times. The figures are:

- GRO-1514: Sample B—charcoal collected by the Abbé A. Glory of Strasbourg in association with the "palettes de couleurs" found in the passage. . . .
8,060 \pm 75 yrs.
 (6,110 B.C.)
- GRO-1182: Sample B—charcoal collected by the Abbé Glory in a hydrocalcite layer on top of a human femur found at the entrance to the cave
8,270 \pm 100 yrs.
 (6,320 B.C.)

Comment. Certainly these two dates have no relation to the Upper Palaeolithic paintings. However, they agree with the recent findings of leaf impressions of *Corylus* and *Quercus* in Layer C of the cave, as De Vries has pointed out.

In retrospect, one admits without hesitation that the ten radiocarbon measurements thus far determined for demonstrably Middle (seven) and Upper (three) Magdalenian sites in France and Northern Spain, excluding Lascaux, are not all in agreement with the geological and paleontological dating evidence. Nevertheless, there are certain groupings which seem to be significant, suggesting that, on the whole, the Middle Magdalenian roughly covers the span *ca.* 14,000 B.C. to *ca.* 12,000 B.C., while it is very probable that the Upper Magdalenian had drawn to a close some time before approximately 10,500 B.C., the end of the Bølling Oscillation (= Early Gotiglacial), when it was already giving way to the Early Azilian development. On this basis the Magdalenian III Stage in France appears to be very close to the same age as the Hamburgian I (Meiendorf: *ca.* 13,800 B.C.) and II (Poggenwisch: *ca.* 13,200 B.C.) Stages of Northwestern Germany (see page 363 for summary), as Rust (1951b: 49, Abb. 1) originally suggested. The dates for the French and Spanish localities are given in Figure 2, while the writer's present views concerning the over-all correlation of the late Upper Palaeolithic succession in the Dordogne within the framework of the Middle-Late Würm sequence in Northern Europe are set forth in Figure 3. The limits of error inherent in this scheme will of course be narrowed as additional adequate and well-documented samples from clearly-stratified localities become available for radiocarbon measurement.

FIG. 2. Radiocarbon dates for Magdalenian sites in France and Spain.

DATES B.C.	NORTHERN SPAIN (CANTABRIA)	SOUTHERN FRANCE (ARIÈGE)	CENT.-WEST FRANCE (VIENNE-INDRE)	N.W. GERMANY (HAMBURG REGION)
7500			• LA GARENNE (L-399 D: 7,550 \pm 500)	
8,000				
8,500				
9,000				
9,500			• LA GARENNE (C-577: 9,159 \pm 480)	
10,000		• LA VACHE (L-336C: 9,700 \pm 200)		
10,500		• } LA VACHE (GRO-2025: 10,590 \pm 105)	• LA GARENNE (C-579: 11,076 \pm 560)	
11,000		(GRO-2026: 10,900 \pm 60)		
11,500			• ANGLES (GRO-1903: 11,970 \pm 80)	
12,000				
12,500				
13,000	• EL JUYO (M-830: 13,340 \pm 700)			• POGGENWISCH (W-93: 13,200 \pm 800)
13,500	• ALTAMIRA (M-829: 13,540 \pm 700)		• LA GARENNE (C-578: 13,897 \pm 1,200)	• MEIENDORF (W-172: 13,800 \pm 800)
14,000				

FIG. 3. The late Upper Palaeolithic succession in the Dordogne within the framework of the Middle/Late Würm sequence in Northern Europe.

DATES B.C.	NORTH EUROPEAN SEQUENCE	UPPER PALAEOOLITHIC AND MESOLITHIC SUCCESSION (S. W. FRANCE)
8,000	POST - GLACIAL TIME	
8,500	FENNOSCANDIAN = YOUNGER DRYAS : ZONE III	LATER MESOLITHIC
9,000		DEVELOPMENTS
9,500	OSCILLATION (ALLERØD) : ZONE II	↑
10,000	OLDER DRYAS : ZONE Ic	↓
10,500		AZILIAN
11,000	OSCILLATION (BØLLING) : ZONE Ib	MAGDALENIAN VIb
11,500		
ca. 12,000	LANGELAND— SAMLAND READVANCE : ZONE Ia = OLDEST DRYAS	MAGDALENIAN VIa Vb
13,000	RETREAT	MAGDALENIAN Va IV
? ca. 14,000	POMERANIAN (>13,000 B.C.)	MAGDALENIAN III
? ca. 15,000	RETREAT	MAGDALENIAN II I
? ca. 16,000	FRANKFURT	UPPER SOLUTREAN MIDDLE SOLUTREAN

The evidence from the Dordogne sites summarized in this paper does not support Gross' (1954: 196-97) suggestion that the Magdalenian VIb development began during Oldest Dryas (paleobotanical Zone Ia) times and persisted throughout the time of the Allerød Oscillation. On this basis, Gross assigns the Early Mesolithic Azilian culture to the Upper Dryas (Zone III) interval, although, as stated above, it appears in Southwestern France at least 1,000 years earlier. In this connection, it seems apparent to the present writer that a new detailed study should be made of the South German localities assigned to the Upper Magdalenian (see Gross 1951, 1954, 1955, and Schwabedissen 1951, 1954, 1957, for references). As Schwabedissen (1957) has pointed out, it would certainly be interesting to have C-14 dates for the Early Azilian for comparison, and to provide a basis for establishing the relationship between the late Upper Palaeolithic and Earliest Mesolithic cultures of France and the Federmesser Cultures (Rissen, Wehlen, and Tjonger Groups) of the North European Plain.

It will be noted that the correlations set forth in this paper are at considerable variance with those proposed by Zeuner (1953; 1959: 339-44). Indeed Zeuner claims that the Middle Magdalenian sites in South Germany are in direct association with terrace deposits which he considers to be of Würm II (i.e., Middle Würm) age. However, this correlation is not convincingly demonstrated, and this seems to be partly due to the fact that Zeuner's concept of the Würm subdivisions differs somewhat from those accepted by other workers. In any case, as stated on page 369, at the late Upper Palaeolithic site of La Colombière, situated in the Southern Jura near Poncin in the Ain Valley, approximately midway between Lyon and Geneva, Judson and the present writer (Movius and Judson 1956) found in 1948 a Final Périgordian (? Périgordian VI) occupation in direct association with a glacial terrace that would appear to be of comparable age with the deposits assigned by Zeuner to the Middle Würm (Würm II) Stage in South Germany. But resting directly on the

surface of this *same* 23-meter terrace of the Ain River at La Colombière, a very typical Middle Magdalenian occupation has been reported by previous workers. Numerous other sites of the same age occur in this section of Eastern France, and many of them actually lie *within* the area that had been previously overlain by ice during much of the time interval which one is inclined to correlate with the main phase of the Middle Würm Stage on the basis of the existing data. For this reason it seems apparent that, although the bulk of the deposits constituting the South German terrace formations which Zeuner has described may well be of Middle Würm age, it does not necessarily follow that the overlying Middle Magdalenian occupations are of comparable antiquity. In fact, only if one proceeds on the basis that this is not true, can the evidence from the nearby sites in Eastern France be explained. In any case, the age of the Magdalenian in this area cannot be at significant variance with its implied age in the Dordogne. The great trouble is that wherever one turns, the confusions and apparent contradictions one encounters are legion. For this reason it is hoped that in the not too distant future the radioactive-carbon method will provide us with some clearly fixed and reliably-dated horizons in Western Europe to serve as fixed time markers for working out the Late Glacial–Upper Palaeolithic sequence.

III. SUMMARY AND CONCLUSIONS

It is apparent that our situation would be infinitely more satisfactory if we had dozens of reliable, radiocarbon-dated samples at hand from the very prolific Upper Palaeolithic sites in Western Europe. Using the information that is available, mainly from certain stations in Southwestern France, and making what appear to be legitimate climatic correlations with the well-established Fourth Glacial sequence of Northern and Central Europe, as set forth in Part I of this paper and summarized in the table (Fig. 1), it now seems likely that:

- (a) The Mousterian Complex lasted for a considerable length of time after the peak of the Early Würm had been attained, and apparently it persisted well into the interstadial (Göttweig) separating the Early and Middle Würm stages, which at the latest began approximately 42,000 years ago (*ca.* 40,000 B.C.);
- (b) The earliest Upper Palaeolithic (Périgordian I followed by "Aurignacian 0") appears in the Dordogne region during the time of the Göttweig Interstadial, which lasted from at least 40,000 to *ca.* 29,000 B.C.;
- (c) The full Aurignacian (Stage I) development began during the severe cold of the early phase of the Middle Würm Stage (? = Stettin) after approximately 29,000 B.C.;
- (d) During the time of the Aurignacian II of the Dordogne there was a temporary climatic amelioration; as yet it is not known if this is in any way connected with the relatively short and weak Paudorf Oscillation (perhaps 2,000 years duration)

when the Périgordian III is found in the loess regions of the north of France;

- (e) The Aurignacian III and IV occupations may possibly be of post-Paudorf age, but their chronological position with respect to the early portion of the main phase of the Middle Würm has never been established;
- (f) The Périgordian IV (Gravettian) dates from *ca.* 22,000 B.C. before the maximum of the Middle Würm had been attained, but no radiocarbon dates for either of the ensuing Périgordian V or VI Stages have yet been determined;
- (g) For the Proto-Magdalenian, which follows the Périgordian VI, there is a firmly established C-14 date of 18,250 B.C. based on seven consistent measurements; apparently this development is roughly contemporary with the time of the maximum advance of the Middle Würm Ice represented by the Brandenburg moraines of Northern Germany;
- (h) During the Aurignacian V, as well as the Lower and Middle Solutrean horizons (which directly overlie the Proto-Magdalenian), the cold dry conditions of the main phase of the Middle Würm persisted;
- (i) The temperate conditions that prevailed during the Upper Solutrean and the Magdalenian I–II occupations of the Dordogne, which are indicated by the pedological evidence and the fauna, suggest a climatic response to the retreat of the North European ice-sheet from the Frankfurt to the Pomeranian moraines, the latter dating from approximately 14,000 B.C. on the basis of the varve chronology and indirect C-14 evidence;
- (j) The cold interval represented by the halt of the ice registered by the Pomeranian moraines seems to correspond in time with the Magdalenian III development, but the evidence for this correlation is by no means conclusive;
- (k) During the relatively temperate time-span of the Daniglacial Retreat the Magdalenian IV and Va sites in the Dordogne were apparently occupied;
- (l) The Magdalenian Vb and VIa cover an interval of intense cold (Oldest Dryas; Zone Ia), corresponding to the temporary re-advance of the Fennoscandian ice-sheet at the Langeland–Samlund moraines; the Magdalenian VIb development took place during the Bölling Oscillation, an early stage in the Gotiglacial Retreat, *ca.* 11,500 to 10,500 B.C.;
- (m) Although C-14 dates are lacking, the Early Mesolithic (Azilian) of this region apparently dates from the end of the Bölling Oscillation and from the short immediately pre-Allerød cold interval when the Older Dryas beds (paleobotanical Zone Ic) were accumulated in Northern Europe.

The post-Göttweig portion of the sequence is clearly marked by two intervals of maximum cold conditions (Aurignacian I and Magdalenian Vb–VIa), and one phase (Proto-Magdalenian) when an intensely severe, sub-arctic climate prevailed. At present, however, it must be admitted that there is no basis for even speculating on when, during the over-12,000-years-long and generally cold to boreal climatic interval represented

by the Göttweig Interstadial, the earliest Upper-Palaeolithic (Périgordian I followed by "Aurignacian 0") manifestations appeared in Western Europe. Certainly before the end of this episode the development of Upper Palaeolithic culture was well under way in the Dordogne region of Southwestern France. The existing data suggest that something of the order of 3,000 or 4,000 years should be allowed for the Aurignacian I and II settlements, and perhaps 1,000 years or even less for the Aurignacian III and IV, while apparently the very much later surviving Final Aurignacian (V) covers an even briefer period. The Upper Périgordian (Stages IV-VI) developments, which intervene between the Aurignacian III-IV and the Proto-Magdalenian, took place during an interval perhaps as much as 4,000 years long. The time-span covered by the apparently intrusive Proto-Magdalenian, which appeared in the Dordogne ca. 18,000 B.C., is unknown. The entire Solutrean development took place during a comparatively brief period of time, perhaps only 2,500 or at most 3,000 years long, between the end of the Aurignacian V

and the beginning of the Magdalenian. However, when more radiocarbon dates are available for sites in this range of time, the guess date of ca. 15,500 B.C. for the earliest appearance of Magdalenian I doubtless will have to be revised somewhat. Consistent with the radiocarbon, geological, and paleontological evidence summarized in this paper and set forth in Figures 2 and 3, it seems likely that the entire Magdalenian development took place during an interval less than 5,000 years long. None of this comes as a surprise, but it certainly emphasizes the magnitude of the acceleration in rate of cultural development that occurred during the Upper Palaeolithic. Although more C-14 controls are urgently needed, at least it is in some measure satisfactory to have some factual indication, however tentative and fragmentary, that the chronology advocated herein is probably reasonably accurate. Future radiocarbon dating will test and correct the possible errors in this tentative scheme.

Comments

By MARTÍN ALMAGRO*

Movius has cited for the Cantabrian Magdalenian III level of the Altamira Cave the date of $15,500 \pm 700$ years, obtained on a sample of carbon. I might just point out that another date for the same level of the same site, ascertained by the same laboratory but on a sample of marine shells, was $13,900 \pm 700$ years.

By A. C. BLANC*

Movius' paper furnishes a most useful and striking picture of the substantial progress made by Paleolithic archaeology in the last decade through the application of the radiocarbon method to the absolute dating of sites and cultures. I would like to offer just a few comments.

1. I would propose dropping the use of the term "Fourth Glacial" for the Würm/Weichsel. It certainly is the Fourth Glacial following the classical sequence established by Penck and Brückner (1909), but we know by now, from evidence collected in the Netherlands, in Northern Germany, in the Alps, in Central Italy, in West Siberia, and also in North America (if we take into account the "Sierran Glaciation," dated by the Potassium-Argon method at older than ca. 900,000 years) that the "First Glaciation" of Penck and Brückner's sequence (Günz) was preceded by an older major cold period (Donau) (Evernden, Curtis, and Kistler 1957). The Würm/Weichsel Glaciation appears to be, as far as we know, the fifth major cold phase of the Pleistocene, each of the five major glaciations being

liable to further subdivision. To avoid confusion, I would therefore prefer the use of the term "Last Glaciation" or "Last Glacial" for the Würm/Weichsel.

2. I may mention a dating obtained by Hl. De Vries from an "Early Würm I" log collected in a peat layer of the Mussolini Canal in the Pontine Plain (Italy), which layer is superposed on a Tyrrhenian II fossil beach with *Strombus bubonius*, and containing a Mousterian industry on pebbles (Pontinian) and an associated fauna with *Elephas antiquus*. The date compares with the "oldest radiocarbon date thus far determined for an Old World sample," also obtained by De Vries (GRO-1397), on the sample Amersfoort XII, attributed to "an Early Würm oscillation separated from the Last (Riss/Würm) Interglacial by a cold period."

The first dating of the Mussolini Canal log was obtained through the normal radiocarbon method (GRO-1353), and the result was: older than 55,000 years (Blanc, De Vries, and Follieri 1957). De Vries then tried to apply the "enrichment method," and in a letter dated August 7, 1959, he communicated a provisional result: "The wood from the Canale Mussolini is approximately 59,000. In 14 days we will have the final result. There was some delay in Amsterdam!" I never received any news of the definite result.

I feel it is useful to underline the similarity in the results of dating the Early Würm I samples from Amersfoort XII (Netherlands) and the Canale Mussolini (Central Italy), obtained as they were from such different geographic areas. These results represent a successful check on the relative dating of the

two sites which had been determined previously on a stratigraphic and paleontologic basis.

I might also mention that it is the Mussolini Canal site in the Pontine Plain, Italy (ca. -59,000 years), and not Lebenstedt, Germany (-48,300 $\pm 2,000$ years), as Movius states, which is "the oldest archaeological site in the Old World to be dated by the radiocarbon method."

3. In his footnote 8, Movius accepts the conclusions of Mme. de Sonnevile-Bordes—reaffirmed in her beautiful work, *Le Paléolithique supérieur en Périgord*—on the necessity of abolishing the term "Périgordian II," which was proposed by D. Peyrony, and of substituting for it the term "Aurignacian 0." In my opinion the problem is still open, perhaps because it has not yet been approached from the right angle. It is difficult for anyone who bears in mind what a careful digger the Abbé J. Bouyssonie is to admit that the co-existence in the Bos del Ser site (near Brive, Corrèze) of "Châtelperron points" and "lamelles Dufour" is the result of a mixture, during the digging, of two (unrecognized) distinct levels (De Sonnevile-Bordes 1960). I visited the site with Bouyssonie: he very positively stated that there is only one archaeological level, and I trust his word.

Now we have a site confirming the validity of the Bos del Ser assemblage of types. This is the Riparo Mocchi in Grimaldi (Italy), where the Upper Paleolithic sequence starts with an horizon containing a lot of "lamelles Dufour" and at least one very typical "Châtelperron point." Having dug this site myself with the assistance of L.

Cardini. I am certain there has been no mixture of distinct levels. The digging was carried out in "cuts" of 10 cm. thickness; the statistical analysis of each cut, now under way, may reveal finer cultural distinctions, but the coexistence of the "Châtelperron point" with "lamelles Dufour" in the same cut is quite certain.

It would be extremely instructive to examine typologically and statistically the assemblage of artifacts discovered by Strobl and Obermaier (1909) in the most important site of Hundsteig at Krems (Lower Austria). I had seen the archaeological material in the Krems Museum in 1936, and had been struck by the mixture of "lamelles à retouches semi-abruptes" (= lamelles Dufour) and typical Aurignacian types. I raised the point with Obermaier, who stated, "One easily gets the impression that at Hundsteig there were two distinct cultural layers, mixed by careless digging. But I have witnessed that there is just one homogeneous archaeological horizon, containing both the retouched bladelets and the typical Aurignacian artifacts."

My feeling is that it is equally arbitrary to call these assemblages "Aurignacian" or "Périgordian," since they contain a mixture of the artifacts most typical of the two cultures. We may be dealing with a typical example of "original polymorphism" of an early phase of the Upper Paleolithic, in which the two cultures (Aurignacian and Périgordian) had not yet segregated and differentiated. This segregation and differentiation appears later, when the "Middle" or "Typical Aurignacian" and the "Upper Périgordian" clearly manifest their typological distinctness. But such distinctness may well have been acquired through the diverging specialization of the two cultures, and does not appear to have been a "primitive character." In fact, the levels in which this mixture of types is apparent are generally older than the ones which are typologically distinct. And where the mixtures persist, we may well have to do with the typical phenomenon of a survival of "original polymorphism," which is the rule in "genetic centers" (Blanc 1940a, 1942-43, 1946, 1957).

G. Laplace, in a still unpublished work pursuing this theoretical trend, has applied to these basal levels of the European Upper Paleolithic the term "synthetotype of the Upper Paleolithic," from the similar term created by Crusafont Pairò and Truysols Santonja (1956) for the early fissiped carnivores, which show a mixture of morphologic characters that are distinct in later genera and species. The recognition of the primitivity of original polymorphisms implies an epistemologic subver-

sion which requires a certain effort but which is highly clarifying for the comprehension of both biological and cultural evolution (Blanc 1940b, 1960).

It would be extremely important, in this connection, to ascertain the absolute age of the Hundsteig site (Krems) by the radiocarbon method. It could easily be done by resuming a limited digging at the site, and determining the age of the archaeological layer just by treating the fossil humus contained in it—a method suggested in 1954 at the Symposium on C-14 Dating in Copenhagen (Blanc and Blanc 1955) and since applied with excellent results both by the C-14 Rome Laboratory (Bella, Blanc, Blanc, and Cortesi 1958-59) and by Hl. De Vries, especially with respect to the loess sites of Austria (see Movius' "References Cited"). The Riparo Mocchi at Grimaldi has yielded a considerable quantity of charred bones, from practically all the archaeological levels, which are going to be dated by radiocarbon. Dating of Krems, Grimaldi, and one of the most typical French sites containing the "synthetotype of the European Upper Paleolithic" (like Bos del Ser) would furnish a definite clue for judging the amount of retardation in the onset of the early Upper Paleolithic as between Eastern, Mediterranean, and Western Europe, a crucial problem of prehistory.

By FRANÇOIS BORDES*

Movius has given us a very fine review of what we know about radiocarbon datings of the Upper Palaeolithic, and a badly needed one. However, I beg to state that I disagree with him on some points, mainly on the general geological and chronological sequence of the Last Glacial.

First, I do not see the advantage of using such cumbersome terms as "Early Würm," "Middle Würm: Early Phase," "Middle Würm: Main Phase," etc. The chances of disagreement between scientists about the meaning of such a term as "Middle Würm: Main Phase" are about the same as if they used "Würm IIIb." It should be understood once and for all that the moraines could well be the worst feature on which to build a chronology of the Pleistocene, since: (1) they usually contain nothing, neither artifacts nor fauna, and when they do contain some, they are dated by them, and not the other way around; (2) they have only local value; and (3) only the ones that were posterior to the greatest extension of the glacier are usually preserved, and these do not tell us much about the beginning of the glacial stage to which they belong.

Anyway, when a person speaks of "Würm" (or Weichsel) today, he has in

mind, unless he specifies otherwise, not any definite moraine, but the last glaciation, and I at least find it much more convenient to speak of "Würm" than of "the Last Glaciation." Within Würm, it is similarly more convenient to say "Würm I," "Würm II," etc., than "Middle Würm: Early Phase," "Middle Würm: Main Phase," etc. When it comes to the older glaciations, the advantage of the names becomes overwhelming. Just think that for "Günz" one has otherwise to use "anteantepenultimate." One cannot say "First Glaciation," since there is the Donau, and no one knows what, before! In French, it is even worse. "Günz" would have to be "l'avant-avant-avant-dernière glaciation"! That is simply not possible.

This side question apart, I disagree on the relative importance of the Würm interstadials. There is no doubt in my mind that a full, bona-fide interstadial took place before the Götweig. Not only is this interstadial clearly marked in the loesses of Northern France, as Commont, Breuil, I, and others have conclusively proved, I hope, but it has been found moreover in Belgium (see Tavernier and De Heinzelin 1957), is clearly to be seen also in the caves and shelters of South-West France, and again in the loesses and shelters of South-East France, as has been shown by the recent work of Dr. Bonifay and H. de Lumley-Woodyear. A. C. Blanc also found it in Italy. If the soil on top of Loess I is often difficult to find, that is not because it was not well developed in Northern France, but because it was heavily eroded at the beginning of Würm II. This big erosional gap is to be found also in several caves and shelters.

The Götweig Interstadial, if we follow Movius (and I rather agree), is to be placed between his Würm I and II, that is, between Würm II and III of Northern France. However, in Northern France it is not usually marked by much of a soil, and I am rather surprised to find that such a soil should correspond to an interstadial 10,000 years long, that is, about the same duration as the Postglacial. If so, it must have been a fairly cold one, with not much vegetation. However, in the caves and shelters, it is clearly marked, as the level in which, as D. Peyrony said, most of the Périgordian I is to be found.

The Paudorf Oscillation is another matter. In the North of France, I cannot see anything representing it except for the slight "pebble band" which divides my Loess III into two parts (Loesses II and III of Movius). That is not much. In the South, the only level which could be a little less cold, and correspond to such an oscillation, would seem to be

the Périgordian V³ level, that is, the Noailles level. And Mövius, perhaps rightly, does not put this level at the time of the Paudorf Oscillation. It has been said, on the evidences of fauna, that Aurignacian II and III had a less cold climate than Aurignacian I. But I do not place too much faith in fauna as indicators of fine climatic changes. Some of the layers of Laugerie-Haute and Combe-Grenal in which there is the greatest proportion of reindeer, for instance, are also the less thermoclastic ones.

On the other hand, I would readily see a true interstadial (between my Würm III and IV) at the level of Middle Solutrean, Upper Solutrean, and Lower Magdalenian (I-III). In these levels, at Laugerie-Haute, there is very definitely a change in the nature of the sediments, the thermoclastic elements becoming relatively rare, and beginning to grow numerous again only in Magdalenian IV.

Also, I am not yet sure of the position of the Late Magdalenian (VI²) and Azilian vis-à-vis the Bölling and Allerød. Anyway, in France we have no trace of any cold oscillation later than the Azilian. It is true that at that time South-West France was far away from the receding glaciers, and that the collium which at Evreux covers the Epipaleolithic (Bordes 1954: 91-99) could be a trace of this last cold period.

I wish to point out also that when I described the poor Périgordian assemblage found in the loess near Paris as "Périgordian III," we did not know that the Périgordian III was in truth Périgordian VI. There can be no doubt now that the assemblage taken from this loess must be older than the assemblage from Laugerie.

By HUGO GROSS*

Part I of Mövius' paper reveals his adherence to the relative chronology of Soergel, which was based on the sequence of cave sediments and on the stratigraphy of the Upper Pleistocene loess found in arid regions of Central Europe. In these regions, several fossil soils register interruptions of the glacial climate by milder and more humid periods. This chronology was introduced into Upper Pleistocene archaeology by Breuil and Koslowski (1931-34), and was recently vindicated by Valoch and Bordes (1957; Bordes 1957). As is well known, Soergel's correlation—of the Young Loess horizons overlying the Krems fossil soil (equivalent to the *limon rouge fendillé* in France) of the Last Interglacial (Riss/Würm=Saale/Weichsel=Eem), with moraines—wanted correction. Above all, Soergel's "Würm I" is, not Warthe, which is

demonstrably the last stadial of the Saale (=Riss) Glaciation (Woldstedt 1929: 169, 184, 207, 242, 313; and especially 1954: 34-48), but Early Würm. Mövius' "Middle Würm" designates Soergel's Würm II+III, whereas Woldstedt's "Mittelwürm" (1958) comprises Soergel's Würm I/II+III+III.

Of course, the Young Loess (Y.L.) does not register the entire Würm (Weichsel) Glaciation. The very humid onset of glacial cooling prevented wholesale and widespread formation and deposition of loess, and this ceased long before the close of the Last Glaciation. Unfortunately, we cannot yet reliably correlate this cessation with any pleniglacial end-moraine, and only suppose that it was the close of the Pomeranian phase, dated by a single varve diagram from a site near Lübeck as 15,730 B.P., rounded off to 16,000 B.P. (De Geer 1954: 310). The time-span between the Pomeranian phase and the Langeland Re-advance is the "Daniglacial" of the De Geers. If we consider the C-14 dates of Meiendorf (W-172) and Poggenwisch (W-93, almost 1,900 years older than the beginning of the Bölling Oscillation) correct (though both were measured by calcareous gyttja), and place both at the close of the Langeland Re-advance because there has appeared no trace of this in either the Meiendorf or Poggenwisch sections, we must date the close of the Pomeranian phase ca. 1,000 years earlier (ca. 17,000 B.P.) because this interval was interrupted by the Belt Re-advance. Otherwise, we must consider the Meiendorf and Poggenwisch dates, and the Langeland Re-advance as well, to be ca. 1,000 years too old. This regrettable uncertainty of course has a serious bearing on the dating of archaeological sites supposed to belong to the transition from the (last) Pleniglacial phase to the Late Glacial. As the coldness, dryness, and length of the Main Phase of the Middle Würm Sub-stage was by far in excess of the other Würmian cold phases, the cessation of the widespread and wholesale formation and deposition of loess was probably more or less synchronous in Central and Western Europe, at least originally.

The following Gotiglacial or Late Glacial (Late Würm) period has been thoroughly studied, by pollen analysis of lake sediments, as to climatic change and internal dating. There were three cold depressions, of which the Older Dryas (pollen zone Ic) and Younger Dryas (pollen zone III) were particularly cold, and the latter, at least, on a worldwide basis. And there were two interstadials, of which the Allerød was

a worldwide synchronous and millennial interval of considerable climatic amelioration that resulted in marked afforestation (except in northernmost localities) with significant floral and faunal change and the formation of peat moors.

Strictly speaking, the intervals of climatic amelioration during a single glaciation, distinguished by Mövius as "interstadial" and "oscillation," are identical, the "oscillation" being a short "interstadial." Likewise, there were short and long stadials. The term "oscillation" ought to be reserved for minor oscillations of the glacial ice margin (e.g., between the main Würmian end-moraines).

The Eem Interglacial came to an end around 70,000 B.P. or somewhat earlier (perhaps ca. 72,000 B.P.), according to the minimal date of 64,000 B.P. $\pm 1,100$ years (GRO-1397) which was obtained on the Amersfoort XII sample taken from a first Early Würm Interstadial horizon. Meantime, it has been ascertained by pollen analysis and C-14 measurement of samples from a single boring that there were two Early Würm interstadials in both Denmark and the Netherlands (Andersen, De Vries, and Zagwijn 1960: 40, 41). The second—Brørup and Amersfoort XIV—ended around 59,000 B.P.; Loopstedt is, on the basis of pollen analysis, also contemporaneous with Brørup. According to the excellent pollen-analytical study by Reich (1953), the formation of the *Schieferkohlen-Flöz* (bed of laminated peat or Pleistocene lignite) at Grossweil near Kochel (not far from the border of the Alps) began in the Riss/Würm Interglacial and continued into Early Würm for two stadials and two interstadials, the second older than 50,000 years by C-14 dating.

The third Early Würm stadial was the Early Würm maximum, the "Würm I" proper of the geological and archaeological literature. The beginning of this stadial is marked by the "cold" Mousterian site of Lebenstedt (Northern Germany), definitively dated by C-14 as 55,000 B.P. $\pm 1,000$ years (GRO-2083, unpublished); the preliminary date was 48,300 B.P. $\pm 2,000$ years (GRO-1219). Tode (Tode, Preul, Richter, et al. 1953), on the basis of the geological evidence and the typology of the stone artifacts (Mousterian of Acheulean tradition, according to him), had already put the Lebenstedt assemblage at the beginning of the Würm Glaciation! It is a nuisance that the prehistorian, Narr, an obstinate adversary of radiocarbon dating (Gross 1959a), subsequently (1959) transferred this assemblage to the period of the first Upper

Paleolithic dispersal in Central Europe *ca.* 23,000 years later, on the basis only of some artifacts made of bone and reindeer antler.

This misleading paper by Narr (1959), disparaging the present writer's C-14 chronology of the Würm Glaciation (1958) which is now vindicated here by Movius, issued from the Würmian date list of De Vries (1958), which unfortunately contained some errors and contradictions. The charcoal of Senftenberg, collected by Brandtner in the upper portion of Y.L. I which was deposited during Würm I (Early Würm) below the clearly-defined Göttweig weathering horizon and was at first dated at 48,300 B.P. $\pm 2,000$ years (GRO-1217), of course does not date the end of stadial Würm I, but an earlier horizon of Würm I. Nor does its age put it at the end of the Brörup Interstadial, then dated at 48,000 B.P. (De Vries 1958: 16), for in 1959 De Vries (Andersen, De Vries, and Zagwijn 1960: 41) made a C-14 determination of 59,430 B.P. $\pm 1,000$ years (GRO-1470) for a stratum near the end of this interstadial. The check measurement of another part of the Senftenberg charcoal (GRO-1771: more than 54,000 years B.P.) is useless, whether the sample was collected below (as Brandtner says) or upon (as Fink says) the Senftenberg loam horizon, because this stratum cannot be the Göttweig loam horizon in this case.

Though the late Professor De Vries assigned the Göttweig Interstadial to the period *ca.* 42,000 to 33,000 B.P. (1958: 15), he (seemingly) identified it, in the same paper (1958: 11), with the Loopstedt Interstadial, which on the basis of pollen analysis (unpublished) is contemporaneous with the Brörup Interstadial. The great thickness of the Göttweig fossil soil (suggestive of a very long process of weathering), though less weathered than the Krems fossil soil formed in the same region during the Riss/Würm Interglacial, has incited not a few geographers, geomorphologists, soil scientists, and Pleistocene archaeologists of Austria and Southern Germany to assign the Göttweig fossil soil to the Riss/Würm Interglacial. Identifiable pollen grains of deciduous trees are very rare in the fossil soils, but are found.

The beginning of the Göttweig Interstadial has been fixed by three C-14 datings: (1) on sample from Upton Warren near Birmingham in Northwest England, 41,900 B.P. ± 800 years (GRO-1245; De Vries 1958: 13); (2) on sample from Oberfellabrunn in Lower Austria, 41,900 B.P. ± 800 years (GRO-1740; De Vries 1959); and (3) sample of a thin interstadial peat layer within Würm gravel, from Hörnating between Mu-

nich and Rosenheim (Bavaria) in the region of the (Main) Würmian Inn glacier (Ebers 1960). The three dates indicate that the Göttweig Interstadial began somewhat prior to 42,000 B.P., i.e., around 43,000 B.P. (cp. Gross 1959, 1959a).

According to the date on the Upton Warren sample, the waning of the Irish Sea glacier (Würm I maximum) began somewhat prior to 42,000 B.P. It went on for many thousands of years until the Late Aurignacian (post-Göttweig), when this glacier re-advanced in Northern England and Wales, though not in the region north of Birmingham. Around 42,000 B.P., a huge dead-ice mass was buried by the waning glacier below ground moraine and glacial outwash near Penkridge (Staffordshire) and did not thaw until the Allerød period *ca.* 30,000 years later (!), thus forming a kettle-hole that was filled by a peat bog (Shotton and Strachan 1959). The fact that this dead-ice mass outlasted the long Göttweig Interstadial proves that the climate of the Göttweig was actually cool, not "interglacial." The present writer is very grateful to Professor Shotton for the important paper cited here and for other publications issued by his Department of Geology, University of Birmingham.

The close of the Göttweig Interstadial is marked, not by the C-14 date on the second humus zone at Oberfellabrunn (GRO-1745: 37,600 B.P. ± 700 years), but rather by a sample taken from the basal part of Y.L. II (GRO-1901: 31,600 B.P. ± 500 years) containing sufficient humus and possibly corresponding to the third (uppermost) humus horizon found there by Brandtner (1954: 58, Abb. 2, 1). This is the case since the interstadial Würm I/II, as determined in other sections and especially in sediments of caves situated 1,000–2,000 metres above sea level in the Alps, came to an end around 29,000 B.P. For example, the Glütschtal lignite, taken from 600 metres above sea level, is 29,000 $\pm 1,500$ years old (B-20)—the probable error of 1,500 years being exceptional for the dates in question! Again, the Upper Paleolithic Olshewian cultural layer ("Aurignacian" II, according to Vértes) in the Potočka cave, 1700 metres above sea level and indirectly dated by the corresponding cultural layer at the Istállóskő cave (535 metres above sea level) in Hungary, is *ca.* 30,710 ± 600 years old. Because of the altitude of the Potočka cave, Penck assigned the Olshewian cultural layer to the Riss/Würm Interglacial (1939: 60), and Soergel, who correctly ascribed it to the interstadial Würm I/II, postulated an almost interglacial climate for the time of occupation (1940). Moreover, the

close of the Göttweig Interstadial is dated on a sample of interstadial loamy Karrestobel peat overlain by Main Würm moraine as 29,000 B.P. ± 500 years (GRO-1260) and 28,840 B.P. ± 300 years (GRO-1277) (Gross 1958: 171), and on a sample from the uppermost peat at Breda in the Netherlands as 29,950 B.P. ± 300 years (GRO-2008) (Andersen, De Vries, and Zagwijn 1960: 40). Movius himself here asserts the figure of 30,000 B.P. Consequently: (1) the basal humous portion of Y.L. II belongs in the Göttweig Interstadial, and the interstadial alternation of humus (black earth) and loess strata upon the Göttweig loam horizon is due to oscillations of humidity (prevailing, at least), not of temperature alone; (2) the cultural strata of Willendorf II, levels 1–4, doubtless belong in the final part of the Göttweig Interstadial, in keeping with the C-14 dates, not in the early phase (Würm II) of Middle Würm; and hence (3) any dislocation of these strata within loess that lies on a steep slope must have been caused by downwash, not by solifluction.

Because the position of the overridden outermost Early Würm end-moraine is still unknown, and because the C-14 figure for the Paudorf Oscillation is so high, Woldstedt and the present writer have placed the main end-moraines of the Weichsel and Würm Glaciations in the post-Paudorf period, the main phase of Middle Würm.

The above subdivision of the Last Glaciation, based on the stratigraphy of the Late Pleistocene loess according to the (emended) theory of Soergel, has been adopted by most Pleistocene archaeologists. However, the archaeologists are matched by only a rather few geologists (in Germany and Austria, respectively, by Woldstedt, Grahmann, and Guenther, and by Brandtner, Gross, and others), notwithstanding that this relative chronology has been verified by the C-14 method, and for this reason these comments have waxed long. Notwithstanding, the above remarks have yet to be supplemented by a review of the Würm heresy.

Unfortunately, this false doctrine, which is perpetuating the long-standing confusion that overshadows Quaternary Geology in the Old World, was inaugurated by the very creator of modern Quaternary Geology, Penck. At first, in 1909, he advocated a tri-stadial Würm Glaciation with the interstadials "Achenschwankung" and "Laufenschwankung," but then, from 1922 on, he assigned both interstadials to the Riss/Würm Interglacial. That this was erroneous is evidenced by the C-14 date of *ca.* 29,000 B.P. for the interstadial Karrestobel peat, which he had at first (1909: 422) placed in his Laufen-

schwankung but subsequently assigned to the Riss/Würm Interglacial, the close of which is now known to be at least ca. 40,000 years older than that peat layer. Moreover, on account of its great altitude, Penck (1939) ascribed the Upper Paleolithic Olschewian stratum of the Potočka cave (placed by Soergel in the interstadial Würm I/II) to the Last Interglacial: this is, of course, incorrect, because nowhere outside of the Mediterranean region have Upper Paleolithic assemblages been found in association with remains of warm-loving interglacial plant and animal species. From 1922 on, Penck advocated the hypothesis of a uniform (*einheitliche*) Würm Glaciation, i.e., a glaciation not subdivided by marked interstadials. Since Penck held Soergel's Würm I/II to be the Riss/Würm Interglacial, Penck's Würm Glaciation has to have encompassed Soergel's Würm II+Würm III+Late Würm. After the introduction of the C-14 method of arriving at absolute dates, Penck's conception has become out of the question, for his "Würm Glaciation" lasted from ca. 30,000 B.P. to 10,000 B.P.! Like most geologists, Penck had not taken into consideration that the accumulation and expansion of the huge Last Glacial inland-ice masses required many tens of thousands of years. And yet, this false doctrine is still being advocated by a number of German and Austrian geologists endowed with sectarian zealotry. If "Young Riss" were actually the last stage of the Riss Glaciation, the so-called "Young Riss Terrace" could not be correlated with Y.L. I of Würm I, for then Young Riss would have to be Early Würm.

The Allerød Interstadial, easily recognizable and very often dated by C-14 measurement, has been repeatedly found at a great many localities in the periglacial areas and in regions glaciated during the Weichsel and Würm Glaciations. Accordingly, it is a justifiable assumption that spatial correlations for other major climatic oscillations of the Weichsel and Würm Glaciations are also valid, e.g., the Fennoscandian of Northern Europe with the Gschnitz Stage of the Alps, and the two Early Würm and Weichsel interstadials. The existence of a threefold subdivision of the Last Glaciation (Würm I, Würm I/II, and Würm II/III) within the areas actually glaciated during the Weichsel and Würm Glaciations has been demonstrated on the basis of field evidence by two Pleistocene geologists—Shotton, in Northwest England north of Birmingham, and Dr. Ebers, near Hörmating in Bavaria within the region of the Würmian Inn glacier. In this latter location, the Würm section can be correlated also

with the threefold subdivision of the Young Loess. This unique section was discovered by Dr. Ebers and viewed by the present writer in 1959, at a deep gravel quarry cut into a large Würm drumlin that is situated ca. 10 kilometres from the innermost (Ölkofen = Pomeranian) Main Würm end-moraine and ca. 23 kilometres from the border of the Alps. Below the Main Würm ground moraine is a decalcified weathering horizon overlying the coarse chalky Upper Würm Gravel, and there is a second decalcified weathering stratum, up to two metres in thickness, between the coarse chalky Upper and Lower Würm Gravels. A C-14 measurement on a sample from a very thin interstadial peat layer (with some *Picea* wood) within the sequence of sediments filling up a basin sunk in the lower weathering stratum and Würm gravel, verified the assumption that the weathering process upon the Lower Würm Gravel set in at the same time as the Götweig Interstadial (ca. 43,000 B.P.). Therefore the upper weathering horizon is very probably a product of the Paudorf Interstadial (see Ebers 1960). The basal portion of the Lower Würm Gravel contains evidence of the two Early Würm interstadials above the *Schieferkohlen* bed of the Riss/Würm Interglacial at Grossweil. There has as yet been no conclusive demonstration of the existence of an Early Würm ground moraine in exposures of this region. According to the excellent Geological Map of Bavaria (edited by the Bayerisches Geologisches Landesamt in Munich, 1954), the outwash gravel from the outermost Main Würm moraine extends along the Isar Valley for 45 kilometres! But the Early Würm glaciers (including the Würm I maximum) most probably did not yet extend out of the Alpine valleys, i.e., the Würmian *Vorland* ("foothills") glaciation did not commence until the close of the Götweig Interstadial. If this interpretation is true, this pre-alpine foothills glaciation alone represents the "Würm Glaciation" in the sense of Penck.

Although there is obviously an urgent need for far more C-14 dates from Northern and Central Europe, we may start now to use the abbreviations

"W I," "W II," and "W III," not for moraines, but for the strongest stadials of the Würm and Weichsel Glaciations, the initial of both of these terms being "W."

In the French and Swiss Alps and their foothills, too, more of the strata that were probably deposited during Würm interstadials ought to be dated by C-14 measurement, at laboratories where the range of the apparatus is at least 50,000 years.

Mövius applies to Upper Paleolithic chronology in France the results of geochronological and paleoclimatological investigations conducted in Northern and Central Europe. He is of course aware of the great, often discouraging difficulties involved in doing so. Certainly the major Upper Pleistocene climatic oscillations were synchronous in France and Central Europe, but modified as to detail according to latitude and longitude, altitude, and kind of archaeological site. As is well known, the development of fossil soils within the *Loess récent* (L.r.=Y.L.) was far weaker in France than in the arid regions of Central Europe. Unfortunately, the numbering of the French Y.L. horizons by Bordes (1957) is different from the Central European scheme (see list of equivalents, Table I).

Mövius points out that Bordes (1957) places the *Solutrén inférieur* of Laugerie-Haute: Est (GRO-1888: 20,650 B.P. \pm 300 years) at the close of his L.r. IIIb. This would mean that the general wholesale formation and deposition of loess ceased in France at the close of the Brandenburg phase (Weichsel maximum), but unfortunately neither this event nor the close of the Pomeranian phase have been dated by C-14 measurement.

In his paper, Mövius has evaluated all the modern excavations of Upper Paleolithic sites in France, examining the archaeological sections for paleoclimatological inferences based on petrology, fauna, archaeology, and, when possible, C-14 measurement. Since the Paudorf interval is not registered clearly in loess sections of regions that are not sufficiently arid (even in western Lower Austria), it can scarcely be ex-

TABLE I

France (and Belgium)	Czechoslovakia
L.r. IIIb	Y.L. III of W III
Cailloutis of "Würm" III	Paudorf Interstadial W II/III
L.r. IIIa	Y.L. II of W II
Sol II/III	Götweig Interstadial W I/II
L.r. II of "Würm" II	Y.L. Ib of W I maximum
Limon brun I/II	Limon brun (?Loopstedt)
L.r. Ib and Ia of "Würm" I	Y.L. Ia of W I

pected to show an influence on cave sediments. The Göttweig Interstadial is often registered in caves by a cave loam stratum and/or by a significant faunal change and bone-decomposition, as evidenced at the rock shelter of La Ferrassie (Deposits E and E').

As for C-14 measurement, Movius has appraised no less than forty dates, by far more measurements than have been carried out with respect to Central European sites of the Upper Paleolithic. Unfortunately a number of potentially important dates are not reliable, because the measured samples were probably contaminated, displaced by burrowing animals, or too little in amount. Above all, there are too few datings of Late Glacial assemblages, e.g., from Late Magdalenian sites, while datings of Azilian sites are totally lacking.

The dates of the Middle Magdalenian occupation layer at La Garenne, measured by the black carbon method, match scarcely at all: probably only C-578 is reliable. After Magdalenian II, there were three cold periods: the Langeland Re-advance + Dryas Ia, Ic, and III; the Bølling Interstadial (cool); and the Allerød Interstadial (cool to boreal). Definitely Post-Glacial conditions occurred in Southwestern France at the earliest during the Allerød period, and in Northern France certainly at the close of Dryas III (Fennoscandian). Obviously something is wrong in the geological dating of the climatic oscillations from the (last) Pleniglacial to the Late Glacial periods. The development of Magdalenian IV-VI probably came to an end somewhat later than is claimed by Movius, and that of Magdalenian VI probably came to an end during the Allerød Interstadial and Dryas III, as evidenced in the Netherlands and Northwest Germany. The present writer's incorrect placement of the start of Magdalenian VIb, in 1954, was based on information from two German Pleistocene archaeologists.

Zeuner's date for the Magdalenian is wrong, because his conception of Würm subdivisions and chronology, based as it is on the solar radiation curve of Milankovitch, is wrong. Further, archaeological assemblages found upon the surface of river terraces can neither be dated by the terraces, which afford only a *terminus post quem*, nor date those terraces. On the basis of geological evidence (Gross 1951; Schwabedissen 1951) and C-14 measurement (H-85-91: 11,300 B.P. \pm 220 years; cf. Münnich 1957: 196), the typical Late Magdalenian site of Andernach-am-Rhine is of Allerød age, thus (in agreement with Soergel 1919, and in contradiction to Zeuner 1953) *younger* than the last general deposition of loess as evidenced by the good preservation of bone and ant-

ler artifacts. However, to repeat, there is urgent need to collect charcoal samples from Late Magdalenian and Azilian sites in France and Spain for subsection to C-14 measurement. The present writer regrets that Movius failed to include an evaluation of radiocarbon-measured Upper Paleolithic sites in the Netherlands and Western Germany, though he understands the reason was that Movius lacked the opportunity to study the relevant assemblages at first hand.

In closing, it should be said that the few criticisms advanced above cannot in the least diminish the value of Movius' admirable achievement in affording us the first comprehensive publication on the C-14 chronology of Upper Paleolithic developments in France, the paradise of *Homo sapiens fossilis*. It is to be hoped that his meticulous treatise will inspire Pleistocene archaeologists working in Western Europe to collect as many more samples as possible for C-14 dating, so that more light may be shed on crucial and completely known periods. The treatise will also be greatly appreciated by Pleistocene geologists particularly interested in the subdivision and chronology of the Last Glaciation.

By SHELDON JUDSON*

Movius' review article represents a monumental task of synthesis and demonstrates a remarkable familiarity with a diffuse and difficult literature. Among all else, it brings out very clearly the interdependence of various disciplines that focus on the study of man's prehistory. As a student of the Pleistocene, I think that the article also indicates that we are approaching rather rapidly the time that the artifacts of man can be used as an index to time, i.e., as a "fossil" in the geologic sense. I have only the following few comments to make.

Relation of Geology to Archaeology. I do not need to belabor the value that geology and archaeology have for one another. I think the record shows a mutually beneficial association, and this association should have continuing value in the future. But Movius' review raises a point that I think should be emphasized.

There are many situations in which the precision of the geologic method does not match the precision of the archaeological record. As an example let us take the Dordogne region, particularly the Vézère drainage and the several classic sites located there, and especially the now-active sites (Laugerie-Haute being excavated by Bordes and the Abri Pataud being excavated by Movius). Two seasons of my own geologic field

work there illustrate that the human chronology being developed by the archaeologists is finer in detail than the physical chronology that the geologist can yet develop. That is, a study of the river regime, both modern and ancient, demonstrates that there has been essentially no change in level of the Vézère from Mousterian time to the present. Thus for these 50,000 years or so the geologist cannot decipher a chronology that might be matched with the changing cultures not only of the Paleolithic but even of the later cultures down to the present day. This is not true in all situations, but it is true in this one, and as yet I see no way of getting a better answer geologically.

Physical Indicators of Climatic Variation. The geologist relies on the interpretation of processes and sedimentation patterns to differentiate fluctuations. In some instances the interpretation is clear-cut. For example, a glacial till obviously means a glacier environment, and loess indicates wind action and its attendant climate and physical environment. But it is not always so simple. Let us take a concrete example.

The deposits within the rock shelters of the Dordogne area are characterized by massive accumulations of rock rubble shed from the walls and ceilings of the shelters. These are usually interpreted as the reflection of climate more rigorous than the present, when frost action was more active than that of the present (cf. Movius). I have myself interpreted similar deposits in the French Jura in this fashion, and in truth most geologists and archaeologists look upon these deposits as good climatic indicators. On the other hand, recent observations by John Miller, Department of Geology at Harvard University, throw some suspicion on this interpretation, particularly in the Dordogne area. He observes that the rock cliffs in the vicinity of Les Eyzies and particularly of the Abri Pataud are currently shedding rock fragments during the colder months. Quantitative measurements suggest that the rate of accumulation of rock rubble is very rapid when viewed against the thousands of years available. In fact, preliminary computations indicate that there has been more than enough time to collect the rubble of Paleolithic times now found within the shelters at the rate at which this material is being shed from the cliffs today. Qualitative observations of my own confirm Miller's observation. All this raises the question as to whether rubble accumulations—the *éboulis* of the French—are in truth a reflection of a more rigorous climate. The question is not settled, because production and preservation are two different things. It

may be that the rock fragments are not preserved under the modern climate but are preserved under some past climate. The point is, however, that the interpretation of *éboulis* is not an open and shut case. I think we should not depend too heavily upon the usual interpretation of *éboulis* until we have settled the question.

There are other areas where we can definitely go wrong. Some "buried soils" are not actually soils. It takes more than a field inspection to determine whether they were formed by surficial soil-forming processes or by some subsurface process. Granulometric studies are very helpful at times, but some of us have become a bit overenthusiastic and have pushed the techniques beyond their present range of applicability.

The burden of my argument is not to berate but to emphasize that the geologic interpretation is not always an easy one. We must continue to be healthily skeptical.

By VOJEN LOŽEK and JIŘÍ KUKLA*

According to our most recent investigations, the following should be noticed about the sediments of the Last Glacial in Czechoslovakia.

The sequence of sedimentation, and the sequence and appearance of fossil soils, are very similar in the famous, often described loess sections at Sedlec near Prague, at Dolní Věstonice, and in the Brno region in Moravia. The loess sections in all these areas may be briefly described, from the surface down, as follows:

(1) Most probably two independent loesses (cp. Young Loess III).

(2) Pseudogley-like, relatively humus-deficient, decalcified fossil soil (cp. Paudorf Oscillation).

(3) Two loesses divided by a slight, humus-deficient, darker-coloured, and partly decalcified zone (cp. Young Loess II).

These three main horizons form the major part of the sections in pure "young" loess, and the intercalated zones of sedimentation are only slightly marked.

(4) When fully developed, a very complicated sequence of humous soils, mostly composed of: (a) three thin, slightly-developed horizons, lying in the upper part of the sequence and divided by loess or by colluvial sediments; and (b) two thick, humous soils at the lower part of the sequence, the lower one of the two being underbedded by a *Verlehmungszone* of ochre-brown colour that could be an independent soil. It should be pointed out that this ochre-brown zone never bears the marks of *Braunlehm* plasma motion (= lessivation)!

(5) Loess which is at maximum a few meters thick and which is interrupted by colluvial sediments (the same kind as are characteristic also for soil complex 4). This loess has often been noticed by earlier authors as impure, the impurities corresponding to some thin humus interlayers which divide the lower part of this horizon, and one of which can also be found close under

the *Verlehmungszone* of the soil complex 4.

There are many localities in Czechoslovakia where this loess is of very little thickness. Nor is there any evidence that the pure loess of this horizon has greater thickness anywhere in Bohemia or Moravia.

(6) Humus horizon lying on the independent soil of *lessivé* type. Here, at the base of the humous horizon or close above the *lessivé*, we have found molluscan faunae of interglacial character.

On the basis of the shallow depth of the loess noticed above under (5), the Last Interglacial together with the following Würmian Interstadial—(6) and (4)—have been described at Dolní Věstonice as one warm period of interstadial character under the designation "Stillfried A." The loess and fossil soil sequences of Stillfried A type in Czechoslovakia therefore include both the Last Interglacial (Eemian) and the Göttweig Interstadial, as these are defined in Movius' paper.

There are some places in Czechoslovakia where the Older Loesses or soils have been described as Würmian or Riss/Würmian. One of them is, e.g., in Sedlec, where the soils of the Penultimate Interglacial (Mindel/Riss, Holstein) were described by Lais (1951) as "Würm I/II."

In the area of Brno, Musil, Valoch, and Nečesaný (1954) and others constructed a loess chronology with a twofold Riss/Würm divided by the "Praewürm" loess. The Würm I loess of these authors is mostly very thick. According to our new investigations, and without the slightest doubt, their Würm I loess is older than the Last Interglacial (Eemian), and their twofold Riss/Würm is really the twofold Mindel/Riss.

We have learned from this case how dangerous it is to "count from the top." It is necessary to investigate carefully the geomorphological position of the section under study (e.g., above which terrace is it situated?) and to use all the classical methods of stratigraphical geology.

By LÁSZLÓ VÉRTES*

Because of the very short time available, I would like to comment briefly on only a single point of Movius' brilliant and comprehensive article—a point that is of particular interest to me. This is the problem of the "Aurignacian I" and "Aurignacian II" finds at the Istállóskő cave.

My determination of the archaeological horizon I call "Aurignacian I," based on pedological and palaeobiological considerations, was that it was interstadial. Its implements were to be found in a vertical distribution of approximately one metre, its most char-

acteristic archaeological finds being the typical Aurignacian bone points (*pointe en os à base fendue*). Considering the fact that this implement type, which can be determined with particular reliability (the first "standard" bone implement type in Western Europe, too), had been until then found only in Aurignacian I, I determined the find as Aurignacian I despite the fact that its silex implements are not exactly similar to those found in the classical Aurignacian I (the characteristic end-scrapers and burins are lacking, etc.). This silex industry is not characteristic, but, since it consists largely of genuine blades, it is nevertheless typically an Upper Palaeolithic industry. There are no archaic forms in it, as some authors have mistakenly suggested. It was from material of this level that De Vries arrived at the radiocarbon date GRO-1501, which is without doubt wrong because of organic contamination (not only because I wrapped the sample in cotton wool in 1949, but also because the ground of the cave, used as a stable for centuries, was permeated by organic substances of a more recent date which could not be removed on account of the smallness of the sample).

I have tried to resolve the contradiction between the standard tool types which are characteristic of Aurignacian I and the uncharacteristic silex implements by regarding my finds as an immediate preliminary to, or primitive manifestation of, the classic Western European Aurignacian I, which assumed its "final" form due to influences absorbed during its westward migration. For the very reason that I wanted to avoid further terminological confusion, I did not give the finds a new name. Nevertheless, the addition of the qualifier "Eastern European type"—with its chronological implication—to the name "Aurignacian I" seems essential for proper designation.

Above this level, there are the remnants of a more humid and perhaps less boreal sterile level and of an abrasion level. In the less humid but more boreal level above this, in a vertical dispersion of about 1.0 to 1.3 metres, lie the implements of the horizon I call "Aurignacian II." Its leading type is the bone point of Mladeč type. Its silex implements are of a pronounced Mousteroid character, but it has also characteristically Aurignacian forms. There is such a substantial typological difference between the archaeological materials of the two levels that I cannot accept Movius' contention that "it would appear that Vértès' 'Aurignacian I' represents an earlier manifestation of the same development." In fact, I have to

suppose that there were even ethnic differences between the makers of the implements belonging to the two horizons.

I identified this upper horizon, which is undoubtedly the same as the Central European Olschewian, with the Aurignacian II or its undeveloped preliminary for two reasons: (1) it was above the horizon characterized by the Aurignacian bone points, and its leading bone implement, the bone point of Mladeč type, is closely related to the leading implement of the French Aurignacian II, the *pointe en os losangique à section ovale*; and (2) the authors created the name "Olschewian" because they regarded it as a stage belonging to the *sensu lato* Aurignacian group, which, however, was prior to Aurignacian I, and is thus the oldest Central European Upper Palaeolithic horizon.

The radiocarbon date of the Aurignacian II site in the Istállóskő cave (GRO-1935), which was based on a sufficient quantity of uncontaminated sample, can be accepted as correct. It is debatable, however, whether the chronological difference between the two horizons actually spanned 5,000 years, as I supposed on the basis of the stratigraphical dates.

It seems at any rate necessary—as Movius proposes—to examine the position, the local differences, and the identicalities or analogies of the archaeological industries denoted by the name "Aurignacian," which has become today merely a loose group designation.

By H. E. WRIGHT, JR.*

The geological and archeological literature on the late Pleistocene chronology of Europe has always been prodigious and forbidding, especially to those who cannot readily study or even scan the German and French papers or do not have the background to follow the obscure names, places, and problems that are mentioned so profusely in many research reports or discussions, which often contain no maps or general descriptions. The stimulus of radiocarbon dating has resulted in a flood of comments, revisions, and discussions, while the reports of new geologic field work, excavations, and paleontological and pollen-analytical studies continue to come, many of them not yet affected directly by radiocarbon dating. For those whose principal language is English, Zeuner's comprehensive summaries of the European Pleistocene geology and archeology have been of great use, but unfortunately the correlations have been colored somewhat by attempts to fit an absolute chronology based on astronomic theory. Now with radiocarbon

dating we can have an absolute chronology without reference to astronomic theory, and the intensity of the climatic changes inferred then depends on the proper interpretation of the geologic and paleontologic evidence.

Summary of even the more recent of this vast literature is a very large task, and few writers in English have both the archeological and geological background to undertake it. The geological sequence is most ample in Germany and Austria, and the archeological in France. Movius, who is a bibliographer of great experience, knows the French problems from his own field work and has been aided in the German literature by the massive almost annual reviews by Gross, who has pulled together a tremendous accumulation of pertinent Central European references, and by the authoritative though less detailed summaries by Woldstedt. He has also been aided by his personal contact with many of the active workers in the European Pleistocene, especially De Vries of Groningen, who produced two-thirds of the 75 C-14 dates discussed in the paper. The review is very timely at this point because the framework of most of the chronology is generally being confirmed and the stage of refining is in sight. The pace of investigation in this field has been very rapid largely as a result of the efforts of De Vries during the last three years, as illustrated by the fact that a similar review, prepared and submitted by Movius for publication at the IV International Quaternary Congress (Madrid) in 1957, but unexplainably not yet published, is eclipsed by far by his present effort, both in scope and detail.

Perhaps the most significant geological aspect of the radiocarbon chronology is the finding that most of the loess of both Northwestern and Central Europe predates the maximum advance of the ice sheets during the last major cold phase (Würm, Weichsel), and that the stratigraphy of the loess deposits and associated archeological horizons reveals a datable sequence of climatic fluctuations not recorded by the glacial features either in Northern Europe or the Alps. Most of the radiocarbon dating of these older materials has been accomplished on the loess sections of Austria and on the peat-bearing sediments of the subsurface of Holland and adjacent areas. Firm correlation between the North and the South has always been difficult and still remains a problem even with C-14 dates. It is therefore unfortunate that, in the interests of simplification and generalization, Gross and many other European authors have applied terms from the South (Göttweig, Paudorf, etc.) to deposits in the North, and Movius, al-

though mentioning the problem, does not criticize this usage, even when he shows that radiocarbon dates as well as archeological stages indicate that Young Loess I, II, and III of northern France do not correlate respectively with Young Loess I, II, and III of Austria.

The insecurity of correlation of the early phases of the Weichsel cold period of the North (equivalent to Tübingian in the Netherlands) with the early phases of loess deposition ("Early Würm") in Austria is illustrated by pollen evidence for two warm oscillations in the North (at Amersfoort, Loopstedt, and Brörup) that are younger than the Eem (Last Interglacial) yet older than 50,000 years. These oscillations, however, are not recorded in Young Loess I of Austria, which presumably spans this time interval because it rests on the Krems soil (=Eem on the basis of the pollen diagrams) and is older than the Göttweig interstadial (which started ca. 42,000 years B.P.). It must be emphasized that to secure reliable dates older than 35,000 years B.P. we require samples that are exceptionally clean of contaminants, and there are thus far very few dependable dates for this older time range—all from the De Vries laboratory.

I should therefore make more of a plea than has Movius for the use of local names until such time as the overall climatic chronology is more definite. Correlations between the Alps, the Southern loess region, the Northwestern loess region, and the North European glacial region can be suggested by tables. In this way the terms would not be so freely mis-used (e.g., Würm I, II, and III). Even the substitution and redefinition as "Early," "Middle," and "Late" Würm does not face the problem that the term "Würm" was originally applied by Penck and Brückner to the moraines and outwash plains of the Alps, and that its application to the loess area and elsewhere by Soergel was an extension not fully justified by the field relations, as forcefully stated by Weidenbach, Büdel, and others.

The major portion of Movius' review concerns the manner in which the Upper Paleolithic sequence, especially in France, fits the general European climatic chronology that is based on the geologic and paleontologic relations controlled by the radiocarbon dates. I am in no position to evaluate in detail this section of the paper, because it concerns in large part the geological analysis of cave stratigraphy by various French archeologists. I should only suggest continued caution in the climatic interpretation of certain cave deposits like *éboulis secs* ("conglifraacts"), stalagmites, red earths, etc. Movius describes in a footnote the implications of

éboulis secs according to the French usage, and accepts the usual interpretation of their genesis. Although some confirmation of the climatic interpretation of the deposits comes from the fauna, it is unfortunate that there are no C-14 dated sites in the Dordogne with both polleniferous deposits and external geologic context, so that a more direct and statistically-based evaluation and correlation of the vegetation and thus the climate may be made. In this respect some of the loess sites of Central Europe (Willendorf, Unterwisternitz) are in better positions for us to tie together with C-14 dates the geology, soils, faunas, pollen, and human cultures.

Despite the difficulties in making valid environmental inferences from the deposits of many of the French caves, owing in part to the lack of external geological connections and of pollen-analytical data, radiocarbon dating has at least provided an absolute chronology for the cultural stages themselves, and has allowed Movius to make some guesses about the duration of each of the Upper Paleolithic stages—certainly a valid objective of prehistoric research. Where such an absolute chronology is lacking, one must depend solely on the climatic chronology for archeological correlation, and there has often been the tendency to correlate one site with another because they have similar archeological assemblages, even though the localities may be widely separated. Such a tendency should be avoided, especially for the latter part of the Upper Paleolithic when the pace of cultural evolution was accelerating, because it assumes at the outset widespread synchronicity of cultural development. Movius yields to this tendency in one instance, when he correlates the Young Loess II of Austria with Young Loess I and II of Northern France, largely on the basis of the artifacts.

As additional radiocarbon dates are produced, the necessity for using artifacts as index fossils will diminish, and certainly the climatic chronology will be more firmly established on an absolute basis. With Movius' review we have many of the problems highlighted, and it should both stimulate additional radiocarbon analyses of critical sites which have good geological and paleontological context and also encourage renewed field studies of important localities.

Reply

By HALLAM L. MOVIOUS, JR.

At the outset the writer would like to express his thanks to each of his friends and colleagues who has submitted com-

ments on his paper. The comments have been extremely helpful, and in the majority of cases not only supplement the text but also support certain of his views. He finds CA[☆] treatment extremely stimulating and useful, and heartily endorses this policy of CURRENT ANTHROPOLOGY. The preparation of this Reply has not been easy, because the author is in the small village of Les Eyzies (Dordogne, France), where he does not have access to his notes, where research facilities are extremely limited, and where he has had to meet a deadline with no secretarial assistance, not even a typewriter! Consequently he trusts that the reader will bear with him in those instances where precise documentation is lacking. The Editor of CURRENT ANTHROPOLOGY will be informed if and when there become available new C-14 dates bearing on the chronology of the Last Glaciation and the Upper Palaeolithic sequence in Europe which necessitate modifications in the correlations proposed in the present paper.

On comments of HUGO GROSS: In his very painstaking and helpful comments, Gross has contributed a wealth of data bearing on the problem of the chronology of the Würm Glaciation. Of particular interest is the new information on the subdivisions of the Early Würm, which supplements the material presented in the text of the present article. Indeed Gross has included both published and unpublished measurements (e.g., LEBENSTEDT—GRO—2083: 55,000 B.P. \pm 1,000 years; BRØRUP—GRO—1470: 59,430 B.P. \pm 1,000 years), of the existence of which the present writer was unaware. In connection with the date of the beginning of the Göttweig Interstadial, Gross' discussion of the Upton Warren deposits constitutes a welcome addition. Furthermore, he cites a date for the thin peat layer exposed at Hörmating (Bavaria), which is as yet unpublished. Certainly as regards the factual demonstration of the existence of a threefold subdivision of the Last Glaciation (Würm) within the area actually covered by the Alpine ice, Dr. Edith Ebers' (1960: in press) paper describing the Hörmating section will be eagerly awaited. In the meantime, it is important to have Gross' brief account based on his first-hand examination of the deposits at this locality. For the end of the Göttweig, Gross includes the dates for the peat sample from Karrestobel (GRO—1260: 29,000 B.P. \pm 500 years; GRO—1277: 28,840 B.P. \pm 300 years), which the present writer intentionally omitted, due to the fact that he does not consider the geologic age of

the horizon in which the sample was collected to be clear.

Gross is inclined to place the Pomeranian Sub-Stage somewhat earlier than the (?) ca. 14,000 B.C. date suggested in the text of the present paper. Perhaps he is correct, but in this regard it is well to bear in mind that if the Brandenburg maximum occurred ca. 18,000 B.C., then one must allow not only for the Frankfurt Sub-Stage, but also for the retreat intervals separating these events. Here, as the author has stated in the text, there is an urgent need for additional C-14 measurements. But it is apparent that due caution must at all times be exercised; indeed, as K.O. Münnich (*in litt.*, 3.5.60; also 1957:196) has pointed out, the dates for Meindorf and Poggenwisch cited on page 363 of the present paper, both of which are based on gyttja samples, do not agree with the figures for wood, bone, and antler from the same two localities. In this connection, Gross has aptly commented on the difficulties of obtaining accurate dates for archaeological sites that belong to the transitional interval between the Middle and the Late Würm Stages.

Gross considers that the Allerød represents an interstadial rather than an oscillation, although he admits that an oscillation is no more than a short interstadial. Obviously this is simply a matter of definition. Since the Allerød represents a relatively brief interval of temporary climatic amelioration, the present writer has called it an oscillation. The important facts are that it took place between just before 10,000 B.C. and lasted until about 8,850 B.C. during the Gotiglacial Retreat, and that it has been identified at so many localities in Northern and Western Europe that its importance as a key stratigraphic and chronologic horizon can hardly be overemphasized.

Based on the dates for certain Late Magdalenian sites in Southern Germany, Gross objects (again perhaps correctly) to the author's present view concerning the terminal dating of the Magdalenian in Southwestern France—i.e., that the Magdalenian VIb was replaced by the Early Azilian manifestation sometime during the Bölling Oscillation, when temperate climatic conditions prevailed in Western and Southern Europe. Very likely the latter opinion will require modification as more C-14 determinations for occupations covering this time range become available. In the meantime, it is felt that during neither of the brief cold intervals represented by the Older and the Younger Dryas (paleobotanical Zones Ic and III) of Northern Europe

was the climate sufficiently cold to account for the high arctic conditions registered at so many localities in Southwestern France and Northern Spain, which have yielded occupations referable to either Magdalenian Vb or VIa, or both. For this reason, the latter have been tentatively correlated with the Langeland-Samland Re-advance (= Bühl in the Alps), when the intensely cold Oldest Dryas (paleobotanical Zone Ia) formations of Northern Europe were accumulated. On this basis, it is possible that the thin stratum of typical *éboulis* which occurs at certain Early Mesolithic sites in Aquitania (e.g., Sauveterre-la-Lémance in Lot-et-Garonne, and the Abris Pagès and Murat, near Rocamadour, in Lot) was formed during the brief cold interval represented by the Younger Dryas (paleobotanical Zone Ic) horizons of Northern Europe. Admittedly, as pointed out in the text of the present article, this problem can only be solved when additional radiocarbon measurements have been determined for sites falling in the Late Magdalenian-Early Azilian range.

On comments of FRANÇOIS BORDES: Bordes comments on the terminology employed by the present writer with respect to the various stages and sub-stages of the Würm/Weichsel, or Fourth, Glaciation. Since no standard international terminology has ever been adopted, this problem in large measure becomes a matter of individual preference. Consequently, on the basis of his conviction that the use of a terminology based on numbers has in the past led to confusion and misunderstanding, the present writer has employed the system advocated by many German Pleistocene geologists (see page 356). Admittedly, what we are all interested in is the use of terms which insofar as possible are not only logical, precise, and clear, but which will also be understood by other workers. The numerical scheme has much to commend it, but when the numbers themselves have a different significance depending on what region one is dealing with, or what author one is following, then the need for change is obvious. In the final analysis, however, the resolution of this problem falls in the domain of geology: it is not one that can be settled by archaeologists—even those who specialize on the Palaeolithic.

In addition to the evidence set forth on pages 357-58 of the present article, together with the new facts presented by Gross in his commentary, it is interesting to have Bordes' observations on the French evidence bearing on the problem of the relative intensity of the pre-Göteborg Interstadial. Outside of France this is registered at Brørup (Den-

mark), Loopstedt (Northwestern Germany), and Amersfoort: Sample XIV (Holland). Although there was apparently an even earlier interruption (? an oscillation) in the Early Würm, as shown by the data from Amersfoort: Sample XII, which, according to Gross, is supported by Helga Reich's study of the laminated peat exposed at Grossweil, near Kochel (Germany), the so-called "Loopstedt Interstadial" was certainly more important. However, neither the relative durations of these two intervals, nor the prevailing climatic conditions during each of them, is as yet understood. If the Loopstedt was in fact "a full, bona-fide interstadial," in accordance with Bordes' conviction, then this problem—the chronological position of which has only recently been demonstrated by the radiocarbon dating method (albeit only within very broad limits)—is in urgent need of further intensive field investigation, not only at the localities listed above, but also at those registered in the loess sections of Belgium and Northern France, as well as the caves and shelters of Southwestern France and the loesses and shelters of Southeastern France, referred to by Bordes. In this connection, in addition to intensive paleobotanical study, it would be particularly important to collect new samples for C-14 measurement.

Bordes' statement relative to the generally cold climatic conditions which prevailed in France during the Göteborg Interstadial supports the evidence from Central Europe discussed by the present writer on pages 358-61. Although the upper and lower limits of this interval have not yet been satisfactorily established in terms of the radiocarbon calendar, it may well represent a time-span of as much as 12,000 or 13,000 years duration, in accordance with the evidence discussed by Gross—i.e., as much as 2,000 years in excess of the dates suggested in Figure 1. Certainly it would be interesting to know which of the early Upper Palaeolithic developments of the Dordogne is to be correlated with the Paudorf Oscillation, an event which thus far is exceedingly poorly documented in France. Of course Bordes may be quite correct in suggesting that the Périgordian Vc belongs here, but, as he himself would be the first to admit, there is no proof whatsoever that such is the case. It is to be hoped that future research at the Abri Pataud and at other sites in the Les Eyzies region will contribute to the clarification of this problem.

On comments of SHELDON JUDSON: Judson raises points which the present writer feels require further amplification.

First, it is true that our current research in the Dordogne region of Southwestern France has demonstrated that for the Early, Main, and Late Würm sequences in that area, the archaeological sites have provided, and are providing, data that makes possible a finer and more precise chronology than the physical chronology developed by the geologists. It should be pointed out, however, that this is in large measure a function of the type of field work conducted by the archaeologists, who excavate sites and thereby are constantly exposing new sections for examination and study. The geologists, on the other hand, of necessity depend in the main on existing exposures which may or may not be strategically located. In those instances where the geologists have actually dug test pits, or made soundings, in buried deposits, new facts inevitably accrue, and this is precisely the type of investigation which archaeologists normally conduct. As to the use of human artifacts as "fossils" in the geologic sense, in connection with research of this nature, the present writer does not share Judson's optimism. For this is only possible in a few rare instances where the regional chronology has been sufficiently firmly established so that the time-space range of the artifacts themselves is clearly understood. At present in the Dordogne, the time when this will be achieved seems even further removed than was true up until approximately ten years ago, when many workers prematurely felt that a reliable archaeological sequence had at long last been established for the Middle and Upper Palaeolithic after nearly a century of intensive investigation.

Secondly, in connection with the interpretation of *éboulis* formations in studies of Pleistocene climate, we doubtless have much to learn. As Judson aptly points out, this is "not an open and shut case" by any means. The salient fact remains, however, that these accumulations are a function of climate, and the *éboulis* deposits encountered in the Middle and Upper Palaeolithic sites of the Dordogne, and elsewhere in France, do differ quantitatively from what is forming today. Qualitatively the latter resemble the older formations in a certain measure, due to the fact that the fracture qualities of the limestone have remained constant. In other words, the process is the same, as Miller contends, but the rate of accumulation has undergone a profound change in response to the inauguration of the Post-Glacial climatic regime. Indeed, no true *éboulis* formations (in the sense the term has been employed in the present article) have ever been reported from archaeological sites in Southwestern France younger than the Early Mesolithic—

e.g., Sauveterre-la-Lémance (Lot-et-Garonne) and the Abris Pagès and Murat, near Rocamadour (Lot). If the claim that "there has been more than enough time to collect the rubble [éboulis] of Palaeolithic times now found within the shelters at the rate at which this material is being shed from the cliffs today" were correct, then the entire mediaeval village of Les Eyzies would now be partly buried and the roofs of the abandoned houses would long since have been smashed by falling blocks of limestone. Certainly, as Judson states, "We must continue to be healthily skeptical," but, at the same time, until more advanced methods of analysis and interpretation have been developed and perfected, we have no alternative other than to utilize the existing data, notwithstanding their shortcomings. Otherwise we will be left with no coherent facts with which to construct a plausible chronological framework for use in connection with correlation studies.

On comments of H. E. WRIGHT, JR. Wright emphasizes the difficulties of correlating Würm/Weichsel events in Northern Europe with those of the Alps, a problem the complexities of which the present writer is only too well aware of. Further, Wright correctly points out that Young Loess I, II, and III of Northern France do not correlate directly with Young Loess I, II, and III of Austria. This latter problem is discussed also by both Gross and Bordes in their comments. Here it is only necessary to add that in Czechoslovakia, as stated on pages 358 and 365, a marked oscillation is in fact registered in the Early Würm Loess (Y.L. I) at several localities (cf. Ložek's and Kukla's comments), and has enabled our Czech colleagues to recognize two zones, designated Y.L. 1a and Y.L. 1b respectively. Presumably further intensive research in Austria will lead to similar results. In any event, the correlation between Northern France and Central Europe proposed by the present writer has not been established "largely on the basis of the artifacts," as claimed by Wright, but rather on the evidence from Godarville (Belgium), where a peat lens at the base of Y.L. II has yielded a radiocarbon date (W-173) of "greater than 36,000 years B.P." In the light of this evidence, it is apparent that Y.L. II in Northern France and Belgium is older than the Göttweig Interstadial, and hence the age of Y.L. 1b in Central Europe. Admittedly, there is an urgent need here not only for additional C-14 dates, but also for detailed paleobotanical studies of the soils and peats associated with the Young Loess and the various subdivisions thereof recognized

in the regions under consideration.

With respect to terminology, Wright makes a plea "for the use of local names until such time as the over-all climatic chronology is more definite." The present writer heartily endorses this proposal, to which he trusts his colleagues in the field of Pleistocene geology will adhere. Furthermore, he is convinced that in Palaeolithic archaeology a similar policy will of necessity have to be adopted, as briefly mentioned in his reply to Vértès' comments.

Wright, following Judson, urges caution in the climatic interpretation of certain types of cave deposits—e.g., *éboulis secs* ("conglifractions"), stalagmites, red earths, etc. The difficulty is that objective studies of sediments of these types are still in their infancy. Indeed, what we badly need, as Wright points out, is a series of intensive studies of archaeological sites, in the Dordogne and adjacent regions, for which C-14 dates are available and which also contain polleniferous deposits and external geologic context. This would certainly provide "a more direct and statistically-based evaluation and correlation of the vegetation and thus the climate." The difficulty is to find such sites. At least some of these essentials are present at the Abri Pataud (Les Eyzies, Dordogne), and it is hoped that interesting and significant data will soon be forthcoming during the course of the excavation of this large early Upper Palaeolithic rock shelter.

On comments of VOJEN LOŽEK and JIŘÍ KUKLA: Ložek and Kukla have contributed a very fine synthesis of the Fourth Glacial succession in Czechoslovakia as it is registered in the classic loess sections of Sedlec (near Prague), Dolní Věstonice, and in the Brno region. It is very useful to have this information, and it is hoped that, in view of De Vries' experience at the Oberfellabrunn brickyard in Austria (see page 360) where the feasibility of dating loess exposures containing fossil soils was successfully demonstrated, additional C-14 dates for the Czech localities will soon be forthcoming. In the meantime, the present writer commends to the attention of his geological colleagues the statement set forth in the concluding paragraph of Ložek's and Kukla's contribution concerning the inherent dangers of the "count from the top" method of correlating loess deposits and the various fossil soil complexes associated with these periglacial sediments.

On comments of LÁSZLÓ VÉRTÉS: Vértès' brief comments are limited to a

consideration of the so-called "Aurignacian I" and "Aurignacian II" of the large and very important Istállóskő Cave in the Bükk Mountains (Hungary). Here the flint implements from the lower "Aurignacian I" horizon, which are associated with bone points with cleft base (*pointes en os à base fendue*), very definitely are not of Aurignacian type. However, as Vértès correctly points out, the fact of the occurrence of a series of true blades indicates that this is a typical Upper Palaeolithic assemblage. Although some of the bone points in question suggest typological affinities with the Aurignacian I of Western Europe, many examples are quite unlike typical specimens from sites in the classic region of Southwestern France. For these reasons the present writer seriously questions whether one is justified in employing the term "Aurignacian" (and even more specifically "Aurignacian I") with reference to the archaeological material from the 1.00-meter-thick lower horizon at Istállóskő (compare De Sonneville-Bordes 1960: 41–150 for a detailed analysis of the typical Aurignacian in Southwestern France). Indeed, Vértès himself seems quite aware of this dilemma, since he now suggests using the term "Aurignacian I of Eastern European type" as a means of more precise definition.

The upper horizon at Istállóskő occurs in a stratum that varies in thickness between 1.00 meter and 1.30 meters. With respect to the affinities of the artifacts found therein, there seems to be considerable terminological confusion. In his report (Vértès *et al.* 1955), Vértès refers them to the "Aurignacian II" on the basis of the occurrence of bone points, which he considers similar to the *pointe en os losangique à section ovale* of the French Aurignacian II, and which he believes were developed from the "Aurignacian I" examples of the lower horizon at Istállóskő. Doubtless he is, in part at least, correct, but admittedly the French analogies seem somewhat vague. However this may be, the salient fact is that the bone points in question from Istállóskő are classic examples of the so-called Mladeč (Lautsch) type, which have a fairly wide distribution in Central Europe, and which are typical of the Olschewian Culture, as originally defined by Bayer (see page 360) on the basis of the artifactual materials from Potočka Cave in Northwestern Yugoslavia. Vértès now states, however, that the associated flint implements "are of a pronounced Mousteroid character, but it has also characteristically Aurignacian forms," and he goes on to point out that he is now of the opinion "that there were even

ethnic differences between the makers of the implements belonging to the two horizons." Thus, it is quite apparent that the present writer's suggestion "that Vêrtes' 'Aurignacian I' represents an earlier manifestation of the same development," based as it is entirely on the published accounts of the Istállóskő materials rather than on a first-hand study of them, should be retracted.

Certainly Vêrtes is correct in his claim that the lower level of the site is the oldest Upper Palaeolithic horizon thus far recognized in Central Europe. However, to regard it as belonging, even *sensu lato*, to the Aurignacian group and hence as being a "primitive manifestation of the classic Western European Aurignacian I, which assumed its 'final' form due to influences absorbed during its western migration," is another matter. At best this notion should be regarded as a working hypothesis which may or may not explain the existing facts. In the present writer's opinion, this assemblage merits a new name irrespective of the consequent terminological difficulties, but it would certainly be premature to propose such a term until after a detailed study and analysis has been made of the entire vaguely defined Central European group of assemblages broadly defined as "Aurignacian." Until they have been subjected to such detailed treatment, their relationship, if any, to comparable materials from Western Europe will never be clearly understood; and the same observation applies to the so-called "Eastern Gravettian" of the Czechoslovakian and Austrian loess stations. This is a big undertaking but a very important one for someone who possesses (a) a proficiency in handling the languages involved, and (b) a thorough knowledge of the West European Middle and Upper Palaeolithic materials.

On comments of A. C. BLANC: There is no question concerning the validity of Blanc's objection to employing the term "Fourth Glaciation" with reference to the Würm/Weichsel complex of Europe. Indeed, as the present writer has stated in his reply to Bordes' comments, he is convinced that the whole matter of Pleistocene glacial terminology is in urgent need of drastic revision, and Blanc's statements only serve to reinforce this opinion. In the meantime, it is hoped that the author's use of the term "Fourth Glaciation," as defined on page 356 of the present paper, will not be misunderstood by readers.

Admittedly the C-14 measurement for the log found by Blanc in a peat layer in the Mussolini Canal was completely overlooked by the present writer, due

entirely to his preoccupation with the radiocarbon dates for localities north and west of the Alps. Blanc claims that the figure in question (approximately 59,000 B.P.) compares with the age of Amersfoort: Sample XII (GRO-1397: 64,000 B.P. \pm 1,100 years); however, the new date cited by Gross for Brørup (GRO-1470: 59,430 B.P. \pm 1,000 years) would seem to be in much closer agreement with the measurement for the wood from the Mussolini Canal. In this connection, it should be pointed out that a recent radiocarbon date for Lebenstedt (GRO-2083: 55,300 B.P. \pm 1,000 years), also given by Gross, indicates that this important locality is in fact some 6,700 years older than originally indicated by the 48,300 B.P. \pm 2,000 years figure (GRO-1219) cited in the present paper.

Blanc's remarks relative to "the necessity of abolishing the term 'Périgordian II,' which was proposed by D. Peyrony, and of substituting for it the term 'Aurignacian 0' " certainly call for further comment. Since Mme. de Sonnevile-Bordes' detailed analysis of *all* the material from each of the so-called "Périgordian II" sites in France has demonstrated that the "lamelles Dufour" always occur in an otherwise typically Aurignacian context, this problem is emphatically not "still open," as claimed by Blanc. Furthermore, Mme. Bordes' observations have been abundantly confirmed by the present writer with respect to the assemblages from those alleged "Périgordian II" sites which he has had the opportunity of studying. Indeed one cannot accept Blanc's evaluation of the Bos del Ser excavations. Here, just as in the case of other localities in the Brive region, Bouyssonie's investigations were conducted with the assistance of students from the École Bossuet at Brive. Due to the fact that at these stations the occupation layers occur in sandy deposits, mixture is inevitable, and one may well wonder if any of these youths could be considered as having been capable of recognizing cryoturbation and other frost action phenomena of the type often encountered at Aurignacian stations in the Southwest of France. But, with specific reference to the subject of excavation technique, one can only speculate on the suitability of the system employed at Riparo Mocchi, where Blanc states, "The digging was carried out in 'cuts' of 10 cm. thickness." On this basis, the occurrence of one Châtelperron Point in an otherwise Aurignacian assemblage, including "lamelles Dufour," seems to the present writer to be of little or no significance. Certainly similar occurrences are not uncommon, as illustrated by the following examples:

(a) ABRI LARTET, Vallon de Gorge-d'Enfer, Commune des Eyzies (Dordogne).—A very typical Châtelperron Point was found in association with an unmistakably Aurignacian I assemblage (De Sonnevile-Bordes 1960: 68–70, 160 and Fig. 96, No. 13);

(b) ABRI CAMINADE, Commune de La Canéda (Dordogne).—Here one Châtelperron Point came to light in the lower of two Aurignacian horizons, while an Audi-type Point occurred in the upper level (De Sonnevile-Bordes and Mortureux 1955; De Sonnevile-Bordes 1960: 109–15);

(c) LES FESTONS, Vallon des Rebières, Commune de Brantôme (Dordogne).—At this Aurignacian I locality, a single point of Châtelperron type has been recorded (Pittard and De Saint-Périer, *Archives Suisses d'Anthropologie Générale* [1955] 20, No. 1–2: 10, Fig. 2, No. 1);

(d) BASSALER-NORD, near Brive (Corrèze).—One Châtelperron Point was recently found at this locality in a typically Aurignacian context (Couchard excavation; Mme. de Sonnevile-Bordes, personal communication).

In this connection, it is of interest to note that at the large rock shelter of La Colombière, near Poncin (Ain), the present writer found two characteristically Aurignacian steep scrapers and several points of Châtelperron type in direct association with a Final Périgordian assemblage (Movius and Judson 1956), and, at the Abri Pataud, Les Eyzies (Dordogne), a very fine example of a Levallois flake turned up in the Périgordian IV layer exposed in the 1953 test excavation (Movius 1954, 1955). Therefore, this author is unwilling to accept Blanc's suggestion that, with respect to those demonstrably Aurignacian sites where "lamelles Dufour" have been documented (= D. Peyrony's "Périgordian II"), "We may be dealing with a typical example of 'original polymorphism' of an early phase of the Upper Paleolithic, in which the two cultures (Aurignacian and Périgordian) had not yet segregated and differentiated." Clearly what confronts us in the majority of instances (excepting, of course, instances of imprecise excavation technique) is no more nor less than the persistence of earlier traits, which, once having been introduced, survived and were employed as long as there was a specific need for them in connection with certain prehistoric activities. But to consider these activities, and hence the traits associated with them, as "synthetotypes" in the biological sense, as proposed by G. Laplace-Jaureteche, is not only naïve and unrealistic in the extreme, but also viciously misleading. Blanc's plea for radiocarbon dates for

Krems-Hundsteig (Danube Valley, Austria), the Grotta Romanelli (Apulia, Italy), Riparo Mocchi (Grimaldi, Italy), and other sites in Eastern, Mediterranean, and Western Europe are certainly to be highly commended, but his view that this will contribute new and important data for evaluating the "synthetotype of the European Upper Palaeolithic" is certainly not substantiated by a rigorous analysis of the archaeological materials from those stratified localities in France which have been competently excavated.

In the final analysis, what Palaeolithic archaeology urgently needs at the present time is less undisciplined speculation and unsubstantiated theoretical constructs, and more well-documented, firmly established facts. Not that theory should be discarded, unless proved to be wrong, but certainly sweeping theoretical generalizations (in many instances accepted as dogma) are unhealthy in any science. Let us hope that the latter have had their day in this field of learning, since the time is long overdue when this kind of thinking should be superseded by a more objective and realistic approach to the fundamental nature of the documents themselves with which the Palaeolithic archaeologist has to deal. Implicitly this field is a humanistic discipline, i.e., one dealing with the science of man, and hence in one's attempts to interpret and order the data one should never overlook the essential humanity inherent in any given prehistoric assemblage.

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Correspondence

► I am completing an article on the controversy over the "surplus" theory of social evolution, in which I defend Harry Pearson's position ("The Economy Has no Surplus" in *Trade and Market in the Early Empires*, K. Polanyi, C. Arensberg, and H. Pearson, eds.) against Marvin Harris' attempt to resuscitate the theory ("The Economy Has no Surplus?" *American Anthropologist*, 1954). Comments from colleagues would be welcomed.—RONALD COHEN, Department of Anthropology, University of Toronto, Toronto, Ontario, Canada.

► I am working on the thread-square (thread-cross, thread-star) and similar objects, and should be grateful for information about its occurrence, form, material, color, occasion of use (e.g., cult of the dead), and its meaning (e.g., a trap for evil spirits), especially from India, Indonesia, Melanesia, America, and Africa. References to its description in the literature are also highly welcome.—HANS E. KAUFFMANN, Silberbachstr. 11, Freiburg i.Br., Germany.

► Is anybody working on the role of witchcraft in the dispersion of populations?—ANDREW P. VAYDA, Department of Anthropology, University of British Columbia, Vancouver 8, B.C., Canada.

► In 1952 we began at Stanford University a project on codification of studies in social and cultural change, with the subsequent aim of developing a more adequate theory of social change. The first step in the project was the systematic collation and codification of empirical studies of social and cultural change. Part of our work has consisted of the compiling of resource materials, including Felix Keesing's *Bibliography of Cultural Change*, which was the initial contribution, and the volume on *Acculturation Abstracts*, covering North America. This is now being enlarged to include work in all areas, and it will be carefully edited for greater usefulness.

We are interested in developing and maintaining as complete a file as possible of empirical studies in the general area of cultural dynamics. This would include material on motivation; structural dimensions of society and culture relating to change and to forms of reactive adaptation; significance of environmental pressures external to groups and cultures (*viz.*, acculturation, disasters, crises, migration, etc.) and relations between individual group culture and environment. I would ap-

preciate any reprints or off-prints by authors for our files.—BERNARD J. SIEGEL, Department of Anthropology, Stanford University, Stanford, California, U.S.A.

North America

► Does anyone have any information about the "Dance of the Lakes" that was taught by Tecumseh during 1811–1812 to the Creek Indians in Alabama and Georgia? Was it associated with revitalization movements in the Great Lakes area; and did it contain any elements identifiable with the Caucasian movement of the Shakers?—CHARLES H. FAIRBANKS, Department of Anthropology, Florida State University, Tallahassee, Fla., U.S.A.

► I would appreciate having any additional firsthand information, however fragmentary, about the snake-handling cult of the southern United States, on which I am writing a book. The origins and early diffusion of the cult are known, but any post-1930 information, ethnographic or psychological, would be useful and valued.—W. LA BARRE, GM Duke Station, Durham, N.C., U.S.A.

► I am currently studying the integration of East Indian settlers in Canadian life, through field work based on a random sample drawn from two cities and four towns in British Columbia. This research is focused on the adjustment patterns of the India-born and Canada-born individuals, particularly on the persistence and modification of old norms, values, and attitudes in the realm of intra-familial relations, kinship ties, economic success, and religion. I would welcome correspondence from Associates interested in boundary-maintaining and boundary-blurring cultural mechanisms among interacting ethnic minorities in North America.—RAM P. SRIVASTAVA, Department of Anthropology and Sociology, The University of British Columbia, Vancouver 8, B.C., Canada.

► I would welcome correspondence about an index to anthropological collections by areas and tribes, which is indispensable to any attempt to revive interest in studies of material culture. It should include separate sections for archeology, ethnography, and physical anthropology, each undertaken by a panel of specialists. A beginning might be made by indexing materials for a study of the arts and industries of the American Indian north of the Rio Grande. Frederick J. Dockstader, Ernest S. Dodge, John C. Ewers, Erna Gunther, W. W. Hill, John H. Rowe, and W. C.

Sturtevant are among the persons most likely to be interested. The panel would employ one or two graduate students to visit and list collections, spending up to a month at the larger museums. National Research Council (U.S.A.) is the logical home for such a committee.—W. N. FENTON, State Education Department, Albany 1, New York, U.S.A.

► I want to start developing a course (and publications by the way) in social dialectology—at first in American English, but to be extended to other language-culture areas as it develops. Bibliography would be welcome!—RAVEN I. McDAVID, JR., Department of English, University of Chicago, Chicago 37, Ill., U.S.A.

Africa

► Since I returned from my 1958/59 expedition to the Central Sudan, I am elaborating the collected linguistic and historical materials on several Central Sudanic tribes—e.g., Dago, Tama, Mimi. Is anybody dealing with similar problems in the same area?—HERMANN JUNG-RAITHMAYR, Sh. el Aziz Osman, 6, Cairo-Zamalek 11, Egypt.

► I am now preparing teaching materials on Kwa languages of West Africa (Yoruba, Ibo, Bini, and Twi). I should be glad to hear from others similarly engaged.—ROGER WILLIAMS WRESCOTT, Social Science Department, Michigan State University, East Lansing, Mich., U.S.A.

► I am at present working on my dissertation, "West African Pidgin-English." Any comments, suggestions, advice, or sources would be gratefully received and acknowledged.—G. D. SCHNEIDER, P.O. Bamenda, Southern Cameroons, West Africa.

South America

► Bibliographical items on the Jivaro and adjacent tribes would be welcome. I am studying the economics of tropical forest peoples, especially those of South America.—PETER VAN EMST, Kon. Julianalaanb, Peterswolde, Netherlands.

Oceania

► For some time I have been compiling one of the largest bibliographies on Samoan culture in existence. I should like to know of any articles that have appeared in non-English journals since 1950.—LOWELL D. HOLMES, Department of Sociology and Anthropology, University of Wichita, Wichita 8, Kansas, U.S.A.

Social Organization of Subhuman Primates in Their Natural Habitat

by *Kinji Imanishi*

INTRODUCTION

IN THIS PAPER I shall review some studies of subhuman primates hitherto undertaken in their natural habitat. I shall not mention the many important studies done in laboratories, zoos, or under similar conditions of captivity. Further, as studies of subhuman primates undertaken in their natural habitat are of too wide a scope and include topics too divergent to be reviewed in this limited space, I shall focus my attention on the social organization of such subhuman primates. Accordingly, I do not describe work done on such topics as food habits, nesting habits, mating habits, territoriality, nomadism, and vocal communication, when it does not concern those problems in social organization that are of interest here. Finally, I shall deal only with certain central problems in the social organization of subhuman primates, namely, the question of

a breeding season, the delineation and definition of the minimum social unit, and some features of the internal organization of such units. Throughout the paper, the focus is on developing theory in the study of the social organization of subhuman primates in their natural habitat.

In connection with theoretical developments, I must draw attention to the progress of method. For long after the days of Darwin, but one simple method was in use. The researcher entered the natural habitat, searched for his animals, and recorded his experiences. This may be called the "naturalistic" approach. Even eminently competent laboratory experimentalists had no more sophisticated method in the field. Field studies done by Zuckerman (1932), Bingham (1932), and Nissen (1931), as also the early works of Carpenter (1934, 1935, 1940), were based on this classical method.

In the early 1940's, Carpenter (1942a) tattooed numbers on many rhesus monkeys, released them on Santiago Island, Puerto Rico, and observed them on the basis of individual identification. This "intensive" method of observing identified individuals, which had already been in use among ornithologists, has brought a great advance in the field study of subhuman primates. For instance, the existence of dominance hierarchy, which had been recognized among subhuman primates under captive conditions, could for the first time be confirmed in the field by means of this method. Carpenter's study of rhesus monkeys, *Macaca mulatta*, was succeeded by the study of Japanese monkeys, *Macaca fuscata*, in Japan.

The study of Japanese monkeys was begun in 1949 according to the naturalistic approach. Since 1952, it has been carried on according to the intensive method of identifying individuals, not, however, by means of marking, but by means of regularly setting out food to attract wild monkeys to come near by. This process, called *provisionization*, has been described by Itani (1954), Imanishi (1957a), and Frisch (1959). Provisionized monkeys are, in a sense, acculturated monkeys, in that provisionization means not only a change in their food habits but also a change in their social behavior and personalities through the contact with man. But they are not captured or domesticated monkeys. They are as wild as ever, excepting as visiting the feed-

KINJI IMANISHI is Professor of Social Anthropology in the Research Institute of Humanistic Science, Kyoto University (Kyoto, Japan). He was born in 1902, and received the D.Sc. degree from Kyoto University in 1940. His long-term field of research is ecology and the sociology of plants, animals, and man, and he is now focusing on problems in the social evolution of primates. As a researcher and explorer, IMANISHI has led many expeditions: among others, to Micronesia in 1941, to the Great Khingan in 1942, to Inner Mongolia in 1944, to Nepal in 1952, to Karakoram in 1955, and to tropical Africa in 1958. He has published numerous books and articles, mostly in Japanese.

The present paper was submitted to CURRENT ANTHROPOLOGY on May 26, 1959, and was sent for CA☆ treatment on July 20, 1959, to seven scholars, of whom the following (plus one who prefers to remain anonymous) responded with comments: A. H. Schultz, H. Harlow, A. Kortlandt, and S. L. Washburn. The comments were sent to the author, who then revised the paper. The revised manuscript was, in mid-April, 1960, again sent out for CA☆ treatment, to four of the original commentators, as well as to ten other scholars from whom comments had not been solicited in the first instance. In response to the second mailing, comments were received from François Bourlière, C. R. Carpenter, M. R. A. Chance, John T. Emlen, Jr., Adolph H. Schultz, S. L. Washburn and Irven DeVore, and Sir Sollie Zuckerman. These comments are reproduced in full at the end of the paper, the references cited by commentators being inter-alphabetized with the author's bibliography but distinguished by the initials of the appropriate commentator. Following the comments is a reply from the author to those comments received in time for the preparation of such a reply. Because A. Kortlandt was traveling in Africa, he was regrettably not available for comment on the final version of the manuscript.

ing place almost every day partially modifies their traditional routes of nomadism. In due course of time, their behavior may come to change considerably, as monkeys born before provisionization gradually die out and are replaced by monkeys born after provisionization and accustomed to man from their childhood. Indeed, as young monkeys which do not fear man and take food directly from man's hand are in fact increasing, provisionization may be a step towards domestication. As Frisch has pointed out (1959), caution on this point is advisable in considering the Japanese studies.

With the introduction of this particular intensive method in the study of Japanese monkeys, another change from historically common research procedure has also been effected. This is the change from short-term observation to long-term observation. For instance, day-to-day records of observation on the monkeys of Takasakiyama, which were provisionized in 1953, have been accumulated by three successive observers continuously over seven years. There is no other example of so long a period of observation of one group of monkeys belonging to one species. And these observations should be continued. Long-term day-to-day observation is now becoming as essential a procedure in studying societies of subhuman primates as it is in meteorology or astronomy.

Where the day-to-day observation of the Japanese monkeys cannot be practiced, observation of at least two or three weeks a year has been carried out. Such year-to-year records of observation are kept for the monkeys of Koshima, Minoo, Shodoshima, and Arashiyama. Individual monkeys of these localities have been named, and the periodic observations are designed to check up on their respective social statuses, sexual activities, and so on. Long-term observation may be the only possible method for intensive survey of societies of subhuman primates, for which neither interviews nor documents can be used by researchers.

If in the following sections of this paper I describe a few new studies of Japanese monkeys somewhat fully, this is not meant to underestimate other studies carried out mostly by the naturalistic approach. It is because the intensive method outlined above has already produced very important results concerning social organization, and thus may be of special interest. The study of societies of subhuman primates will advance rapidly if this method is applied by many more field researchers and to various subhuman primates other than Japanese monkeys.

PROBLEMS

BREEDING SEASON

Some thirty years ago there was no reliable report of systematic observations on the social life of monkeys or apes in their natural habitat, but only anecdotal or fragmental information collected mostly by hunters and travellers. Zuckerman was one of the pioneers in recognizing the urgent necessity for more accurate knowledge on the natural life of these animals. He compiled data of a kind thitherto unknown, and paved the way of a new scientific approach to the study of sub-

human primates. Although most of his observations were carried out in zoos, he did also study the natural life of wild chacma baboons, *Papio comatus*, in South Africa.

Zuckerman was the first person to try to formulate a scientific hypothesis as to the foundation of primate society. This hypothesis was based on considerations of reproductive physiology.

On the 4th of May, 1930, he succeeded in collecting twelve adult females from one troop of wild chacma baboons. He wrote (1932: 50):

The females shot on May 4th were representative of practically all stages of the complete cycle. Some were non-pregnant. Some were in the early stages of pregnancy, and one contained a fullterm foetus. Others again, were lactating, and one of these, judging by the age of her baby, would soon have restarted her normal menstrual cycles. This disparity between the different females proves conclusively that female Chacma baboons become pregnant at all times of the year in the neighbourhood of Grahamstown.

On the basis of this knowledge obtained by himself about the breeding of wild chacma baboons, Zuckerman developed his principle of primate sociology as follows (1932: 313):

The factors underlying associations of monkeys and apes are characterized by their continuous, rather than intermittent, sexual nature. The male primate is always sexually potent, while the female is also always to some extent receptive. In the lower mammal, on the other hand, the female, as a rule, accepts the male only during isolated periods of heat, and this intermittent character of the sexual bond is reflected in the transitory nature of their social unions.

In short, he considered that the factor underlying the permanent association of the sexes was their uninterrupted reproductive life.

Carpenter seems to support Zuckerman's hypothesis to some extent when he says (1942b: 196):

Available facts for most monkeys and apes indicate that they do not have discrete breeding seasons, although the rate of reproduction may vary from season to season. Sexual activity takes place throughout the year in chimpanzees, gibbons, rhesus monkeys, baboon, howler and spider monkeys. The males usually have a succession of estrous females with which they copulate repeatedly during the females' limited estrous periods.—It would seem reasonable to suppose that the relative constancy of primate groups is importantly related during all seasons to the incidence of sexual behavior.

It was the merit of Haddow to prove that there is a breeding season in redbellied monkeys of East Africa, *Cercopithecus ascanius schmidtii*. His data derived not from direct observation in the field but from analysis of a total sample of 47 mature females shot by him. With respect to these, he confirmed the number of females in early pregnancy, and that in advanced pregnancy in each month. He further referred to the record of five living females with infants. Of the results, he wrote (1952: 304):

It is admitted that the sample is a small one, but these results are so suggestive that it is hard to escape the conclusion that in the case of *C.a. schmidtii* there appears to be at least a preferential breeding-season. Thus, taking the time-estimates given above for pregnancy and lactation, it will be

seen that they are consistent with a preferential breeding-season lasting, approximately, from December till April.

Referring to a marked seasonal variation in birth rate in a colony of rhesus monkeys (*Macaca mulatta*) in the Carnegie Institute at Baltimore, which was reported by Hartman (1931) and reviewed by Zuckerman (1932) as not conclusive, Haddow (1952) enumerated the dates of forty births recorded for rhesus monkeys in the Virus Research Institute, Entebbe, Uganda, and suggested that, in this case also, there might be recognizable a preferential breeding season. In both cases the monkeys were kept in captivity, and, so far as I am aware, it has not yet been confirmed that there is a breeding season among rhesus monkeys in nature. Among Japanese monkeys, however, which belong to the same genus *Macaca* as rhesus monkeys, a definite breeding season has been recognized during observations in the field.

The breeding season of Japanese monkeys concentrates in the winter, though the female maintains a regular menstrual cycle of 28 days throughout the year, as observed by Hazama (1954). In Takasakiyama, copulations have regularly occurred from October to April during the last seven years, except for a few anomalies. Accordingly, births there have been observed to occur from May to September of each year (Table 1).¹ The

TABLE 1

INCIDENCE OF COPULATION AND BIRTH IN THE OIKIA OF TAKASAKIYAMA FROM OCTOBER 1954 TO DECEMBER 1955 (AFTER UNPUBLISHED DATA RECORDED BY ITANI)

Year	Month	Copulation†	Birth†
1954	October	*	
"	November	**	
"	December	***	
1955	January	***	
"	February	***	
"	March	**	
"	April	*	
"	May	△	*
"	June	△	***
"	July		***
"	August	△	***
"	September	△	*
"	October	△	
"	November	**	
"	December	***	

† *** = frequent

** = not frequent

* = rare

△ = masturbation, consort relationship, and sexual display, but not true copulation

monkeys under observation at the other localities in Japan reveal the same tendencies. It may be worth mentioning that during the non-breeding season males can never be found mounting females in Takasakiyama.

Japanese monkeys live up to the northern limit of distribution of all subhuman primates. One might think that if they lived in the tropics instead of in the

temperate climate of Japan, they would have no breeding season. However, as already mentioned, there are monkeys, like redtails, which have a breeding season although they live in tropical Africa. On the other hand, Haddow could not find a breeding season in *Colobus abyssinicus*, another tropical African monkey.

It is as yet premature to generalize about breeding season in subhuman primates because there are only a few species the reproductive life of which has been thoroughly surveyed under natural conditions. From our present knowledge, it is safe to say that, though chacma baboons have no breeding season, hamadryas baboons, *Papio hamadryas* (cf. Zuckerman 1932: 56), may have one, and that, though Japanese monkeys have a breeding season, rhesus monkeys may not have one.

As to social organization, it is clear that the factor underlying associations of subhuman primates cannot be sexual relationship alone, as Zuckerman stated, since among monkeys which have a definite breeding season there is no loosening of solidarity during the non-breeding season. As Carpenter said several years ago (1942b: 198):

To conclude, however, that sexuality is the only enduring basis of these societies would be to over-simplify the complex facts. For long periods of time in free ranging primate groups, no primary sexual behavior can be observed, yet neither the group pattern nor size change but these persist and the group remains a coherent social unit.

THE OIKIA

Zuckerman's second principle of primate sociology, as important as his first, is that families including both sexes are the fundamental units of primate societies. Thus Zuckerman wrote (1932: 314-15):

The nucleus of the societies of monkeys and apes is the family party, consisting of an overlord and his harem, held together primarily by the interest of the male in his females and by their interest in their young. Paternal interest is not strongly manifested by sub-human primates, and in this they doubtless show resemblances to the behaviour of the lower mammals. But the family of the primate differs from that of the lower mammal, since one of its essential members is the overlord. The male of most lower mammals separates from his females after his rutting season is past, so that the family in his species is formed by the female and her young. A conspicuous characteristic of the harem system and the system of dominance in the primates is the absence of all but clandestine promiscuity. And in this again the primate differs from many lower mammals. The harem forms the nucleus when several family parties unite to form a larger herd, but the herd never appears to be so stable a unit as the family, which never loses its identity within the larger group.

This generalization of Zuckerman's, from his observations of chacma baboons, is accepted by anthropologists as the standard theory. In this connection, it is worth mentioning that Bolwig, in a recently published paper on the same baboons (1959), says he was not able to discern clearly either harem or family in the bands observed by him in Rhodesia. However, careful observations, though without individual identification, made by Lumsden (1951), Buxton (1952), and Haddow

¹ In the beginning of 1955, the population of the oikia (for definition of this term, see p. 397 of Takasakiyama was as follows: leaders 6, subleaders 10, peripheral males 50, male infants 60, total number of males 126; females 135, female infants 60, total number of females 195; total number of males and females 321. Of the 135 females, 100-115 were sexually mature. Total number of births in 1955 was 59 (male 30, female 28, uncertain 1). These numbers were all recorded by J. Itani.

THE "JAPAN MONKEY CENTER"

SHORTLY after the war, Kyoto University saw the foundation of a Primate Research Group under the direction of Professor Denzaburo Miyadi and Dr. Kinji Imanishi. The group's chief interest revolved around studies in ecology and social behavior. A few years later, an Experimental Animal Research Committee was formed at Tokyo University, whose main concern was the experimental use of monkeys for medical research. Cooperation developed between the two groups and eventually, in 1956, brought about the formation of the Japan Monkey Center. This organization includes at present 15 researchers, and its officially published objectives are:

- (1) To foster team-research on primates.
- (2) To study intensively the ecology and social structure of populations of *Macaca fuscata* still living in Japan, and to work out methods for the conservation and propagation of these populations.
- (3) To establish and operate, in cooperation with several universities, a primate laboratory and to centralize the data obtained by students in these universities.
- (4) To establish breeding colonies of Japanese monkeys, with a view to supplying the growing demand for experimental animals.
- (5) To open a primate zoo as a means of studying problems connected with human origins.
- (6) To publish books and journals on the study of primates.

... Close-range study of wild-monkey populations, made possible by painstaking provisioning, so far forms the most significant contribution of the Japan Monkey Center to primate research. However, the work of the Center is far from being limited to these ecological and sociological interests. Two breeding colonies have been created. In one case a natural population was captured in its entirety and released on a small island. The other group was artificially formed with monkeys captured one by one in Yakushima. The integration of the new group was achieved in captivity

by a method which allowed researchers to observe some of the mechanics of group formation. . . .

Last year, members of the Center organized two field trips, one to central Africa and the other to South-East Asia. Their objective was twofold: first, to examine how primate ecology and social behavior may be studied in these regions and to see to what extent the method of provisioning, so successful in Japan, could be used in a different environment; secondly, to make contacts looking to the importation of several primate species to Japan, either for transplantation in Japanese islands or for building up the primate zoo, which is one of the objectives of the Center.

Already three gibbons, 18 crab-eating macaques and six red-faced monkeys have been released on one of the numerous uninhabited islets scattered along the shore of Japan.

Although the Center itself has some equipment for physiological and anatomical research, most of the laboratory work, especially that connected with medicine, is carried out at the various universities connected with the Center. The results of these studies are collected and made available to all students at the Center.

The first issue of a journal of primatology, *Primates*, appeared in 1957. . . . It has been announced that, thanks to a grant of the Rockefeller Foundation, the third and fourth issues would be published entirely in English. It is hoped that this will constitute the first step in creating an international organ for studies in primatology.

If the scientific quarterly has suffered from initial difficulties, the same is not true of a lighter magazine, *Monkey*, addressed to a high-school public. This publication, which has already reached its thirteenth number, maintains a surprisingly high standard. Much of the data concerning the development of the supervised colonies can in fact be found only in this high-school magazine. Excellent photographs, in the best Japanese tradition, illustrate the articles.

JOHN E. FRISCH
(*American Anthropologist* 61: 584-85, 592-93)

(1952) in Uganda, seem to support the principle offered by Zuckerman. The following is a quotation from Haddow's paper (1952: 353):

An important observation made by Zuckerman is that *P. comatus* bands split up into small family parties for the night. At a later date Lumsden, working on the sleeping-habits of monkeys in western Uganda, found that this observation held good for various species belonging to the genera *Cercocebus*, *Cercopithecus*, *Papio* and *Colobus*, and the present writer has seen a band of ten chimpanzees (*Pan troglodytes schweinfurthii*) divide into at least two parties for the night. Lumsden's results showed that in *C. albigena johnstoni*, *C. a. schmidtii*, *P. doguera tessellatus* and *C. abyssinicus* the mean size of resting bands was between four and five individuals, and the figures for other less common species were largely in agreement. Buxton, working on *C. a. schmidtii* in a small forest patch in central Uganda, has found that there the mean size of resting bands is even smaller, being between three and four individuals. On the other hand, bands observed by Lumsden during the day were larger, usually numbering over ten individuals.

The largest band of *C. a. schmidtii* reported by Buxton was 26. He considered that the probable composition of the normal family group in this species was one male, one or more females, and their young, as was

found by Zuckerman among baboons. However, if the family formation were a direct derivative of non-intermittent reproductive activity and male dominance, as Zuckerman assumed, one would not expect to find such family groupings among *C. a. schmidtii*, because in this species not only is the breeding season recognizable, as has been mentioned above, but also male dominance is inconspicuous, as both Buxton and Haddow have pointed out.

On the other hand, Carpenter confirms that in groups of howling monkeys, *Alouatta palliata* (1934), and rhesus monkeys (1942a) there are a number of adult males, and more adult females than adult males, yet they have no breeding season, and there prevails among them a type of rotating mateship—a type of promiscuity.

Accordingly, it is clear that Zuckerman's second principle on the family or harem system does not hold good for societies of monkeys and apes in general.

What, then, is meant by the term "family"? Carpenter (1940) described the group of white-handed gibbons (*Hylobates lar*) he observed in Thailand as a "family," it being typically composed of an adult and reproducing male and female, and a succession of young ranging in age from early infancy to late juvenility or early adulthood. Previous to Carpenter's study, McCann

(1933) had described as a "family" the similar composition of the group of white-browed gibbons (*Hylobates hooleck*) observed in Assam. In his report (1951) Lumsden might call a "family" the band of six *Colobus abyssinicus ituricus*, including two juveniles, which was observed by him continuously over three months, if he could confirm the presence of an adult male and three adult females in the band, but he might hesitate to call it a "family" if he found there were two adult males and two adult females in it. When the howling monkeys of Barro Colorado Island were surveyed by Carpenter (1934) in 1933, each clan (or "group," after Carpenter) contained, on the average, three adult males, but when they were surveyed by Collias and Southwick (1952) in 1951, 23 of the 29 clans had simply one adult male, and only six of them had two males; those who did not know the results of the 1933 census might conclude from the 1951 census that a "family" pattern was predominant among howling monkeys.

Reporting his field study of chimpanzees, Nissen said (1931: 18):

In six instances I was able to determine that there were at least two mature males present in a group. These observations certainly do not exclude the possibility that a chimpanzee band is composed of a polygamous male and his family, since a male may well have one or more mature sons, but they do lend some probability to alternative explanations. The extreme possibility, that neither male nor female chimpanzee is limited to a single mate, even temporarily, has never been satisfactorily disproven, as far as I know.

In my opinion, a band in which promiscuity prevails cannot be called a "family" even though the two males in it are a father and his mature son. In other words, we may legitimately consider it a family only if we can establish the absence of incest between this mature son and his mother. It is almost impossible to answer such a requirement without knowing the exact pedigree of each individual which participates in sexual activities in the band. Nevertheless, until such incest is disproved, it is appropriate to restrict the use of the term "family" in scientific description of subhuman societies.

Here I would like to recommend the word *oikia* as a technical term² designating the minimum unit of social life found in any species of animal, regardless of the composition of that unit. Then, with reference to societies of subhuman primates, the clan in howling monkeys, the family in gibbons, the small nesting party or sleeping group in redtail monkeys, and the harem in chacma baboons, would each be describable as an "oikia." The large daytime band of redtails, or troop of chacma baboons, would be, not an oikia, but an aggregate of oikiae.

As already mentioned, Carpenter (1942a) did not recognize in rhesus monkeys the harem or family that Zuckerman found in baboons, though macaques are closely related to baboons in physical structure as well as in mode of life. However, though the group among rhesus monkeys is sometimes as large as the troop of baboons, it appears to be a definite and stable social

unit and never to lose its identity, so that I regard it as an oikia. This judgment applies equally to the group of bonnet monkeys (*Macaca radiata*) observed by Nolte (1955) in southern India, and to groups of Japanese monkeys.

It is worth noting that there are at least two distinct patterns of organization at the oikia level. In the one pattern, each oikia has its own territory and is antagonistic to contiguous oikiae, though it may accept a solitary intruder in some cases. Examples are oikiae of howling monkeys, rhesus monkeys, and gibbons. In the other pattern, each oikia associates easily with contiguous oikiae under normal circumstances, the oikiae together forming a large band or troop. Examples are oikiae of redtail monkeys and chacma baboons. Oikiae which associate to make a large unit seem, however, to live within a certain area. Haddow writes on redtail monkeys as follows (1952: 357):

On any given day these will be found divided into smaller groups, but under ordinary circumstances these groups will be limited to the particular area concerned. Under certain stimuli, notably that of a fruiting tree, they will coalesce, and all the Redtails of that particular area will be found banded together temporarily. While the evidence is not conclusive, it is thought that when such very large aggregations do occur they consist of monkeys from a particular area, which may have fairly definite boundaries.

It may be said that each oikia of redtail monkeys shares a particular area as a common territory, but, within it, not occupying its own territory and maintaining a neighborhood relationship with the other component oikiae of the band.

We have already noted that Haddow (1952: 353) observed a band of ten chimpanzees to divide into at least two parties for the night. Donisthorpe, who made a field study of mountain gorillas in 1957, says (1958: 205):

It has been suggested that gorilla groups keep to their own territories and resist intrusion by other groups. I saw no signs to support this suggestion. Towards the end of March a group of three and a large group were known to be in the same area, moving in opposite directions. By March 31 they had passed each other within 100 yards. Had there been any antagonism one would have expected the smaller group to retreat.

She also says (1958: 216):

Large groups sometimes divide and rejoin, but they do not always split into the same component parts.

Chimpanzees and gorillas may have the same pattern of organization as redtail monkeys. It seems precarious, however, to discuss the organization of a large band or troop without exact knowledge of its component oikia, that is, the minimum social units.

INTERNAL ORGANIZATION OF AN OIKIA

CLASS AND SUBGROUP

Carpenter assumed the existence of "subgroups" within the large group of macaques observed by him, though he did not afford a concrete example of the

²This term was first proposed by the author in 1950. At the same time, he proposed the term *specia* to designate the maximum social unit, that is, the total society of any species of animal, including in number all individuals of the species, and covering in extent the whole distribution of the species.

organization of such subgroups. For instance, he said (1942b: 188):

Let us assume that through various behavioral exchanges, involving avoidance, maturation and learning, two males of a primate group become by degrees strongly antagonistic. At the same time, through reciprocal motivation and affiliative learning, several females with their young are attached to the more dominant male and several females and young likewise attach themselves to the male which has a lower dominant rank. The males will tend to avoid each other, and, while doing so, will carry with them their attached dependents. Then, as the *intra-sub-group* bonds increase in positive strength and the *inter-sub-group* relations weaken or increase in antagonistic quality, the sub-grouping relationships change from that of being part of a larger group to a status of an independent new group. The males act like polar bodies in cell division.

Altmann (1959) calls "subgroup" the association of a female and its dependent infant in an oikia of howling monkeys, but in this paper I limit discussion of the term "subgroup" to the definition originally advanced by Carpenter.

Carpenter also used the term "class," to denote various sex- and age-groupings within the same large group of macaques. These "classes" had differential functions in the social organization of the oikia:

... The control of a group is distributed among individuals and classes of individuals in direct proportion to the statuses of individuals or classes of individuals [Carpenter 1954: 274].

It is interesting that, in the course of field studies of Japanese monkeys, class organization and spatial distribution of classes attract the observer's notice earlier than does subgroup organization. This may be due to the fact that class organization is a more basic ingredient of oikian social life than subgroup organization, which stretches over classes. Generalizing from several oikiae intensively observed during recent years, it seems reasonable to recognize at least three main classes in the social organization of Japanese monkeys, namely, (1) central males including leaders and subleaders, (2) peripheral males, and (3) all females together with dependent infants. Females may be divided into central females and peripheral females, as in the case of males. But in females the cleavage between the central and the peripheral is not so distinct as in males.

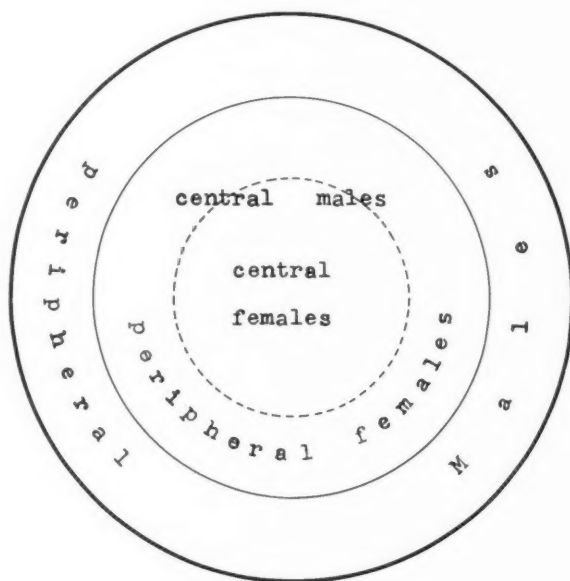
In a preceding paper (Imanishi 1957a), I included a schema that showed the spatial distribution of *oikions* (that is, the individual components of an oikia) after the studies by Itani (1954) of the oikia of Takasakiyama. Now I can rewrite that as a generalized schema representing spatial distribution of classes in any oikia of Japanese monkeys (Fig. 1).

Leaders and subleaders seem to make up separate and discrete classes in such a big oikia as that of Takasaki-

³ In the oikia of Takasakiyama, difference in social status between leaders and subleaders is distinguishable as follows: when leaders are in the central part together with females, infants, and babies, subleaders cannot enter there, though they usually situate near the central part. When leaders retire to the mountain but some females, infants, and babies remain at the central part, subleaders at once enter there and take the leaders' role; females which chase intruding subleaders when leaders are present then tolerate the presence of subleaders. As for age difference, leaders of the oikia are estimated to be over twenty-five years old, while subleaders are estimated to be around twenty years old.

yama,³ but in smaller oikiae, where the total number of leaders and subleaders is also small, they may be seen to belong to one class of central males. When Carpenter says of rhesus monkeys that, "Some of the females, usually the more dominant ones, may form a kind of central cluster around the most dominant male but the relationship in such a sub-grouping is easily differentiated from the male-female consort relationship formed by an estrous female with a responsive male" (1942a: 139), it is evident that this subgroup derives from an intimate relation between a central male and some central females based on their common spatial occupancy in an oikia. Furuya, who studied such subgroups of Japanese monkeys in the oikia of Takasakiyama by analyzing grooming relations among oikions, has reported (1957) that some leaders of the oikia make more or less definite subgroups with attendant females. One of the reasons why such subgroups

FIG. 1. Schema representing spatial distribution of classes in an oikia of Japanese monkeys.



do not establish independent units in the oikia may be the strong tie conditioned among males which make up the leader class.

DOMINANCE RANK

One factor supporting the integration of an oikia is the dominance rank or dominance hierarchy settled upon and sanctioned among oikions. From his observation of rhesus monkeys Carpenter has said (1942b: 194):

Usually, several group males which are arranged in a dominance hierarchy are competitive in some situations and with certain degrees of motivation, while the same animals may closely coordinate their behavior in other situations with different degrees of motivation. For example, the male despot of a rhesus monkey group will exclusively possess a tray of food until he is satiated, while all the other animals wait. Yet, he

will closely coordinate his behavior with the rest of the group males during an inter-group fight or while leading the group from one location to another.

Dominance rank is easily detected experimentally by repeating a simple test designed to show which one of two animals will take a given food.

Itani (1954) found in Takasakiyama that there were individual statuses determined by the dominance rank within each class of males, and that each class itself also had a status determined by the dominance rank among the different classes of males, the leader class having the highest status. The dominance rank is usually linear.

Dominance rank is not confined to males but is found also among females and immature animals. Carpenter has written (1942c: 253):

This does not preclude the fact that, among the females, as among males, there is also a dominance gradient. As compared with that of the males, this female dominance gradient is of a lower slope, i.e., females have less absolute dominance than males and they seem to differ to a small degree. Nevertheless, the female gradient overlaps that of the males. This means that some of the most dominant females are more dominant than some of the least dominant males.

When dominance is studied in immature animals it is found that there, too, an *unstable* gradient exists. Indeed, the status of an individual *Rhesus* monkey begins to be defined during its infancy, after it has begun to engage in social play.

The existence of central and peripheral females in the spatial organization of an oikia rightly suggests that there is this dominance rank among females. However, as Carpenter has not given any concrete example of dominance rank among females or immature animals, the observations by Kawai on the oikia of Koshima (1958) and those by Kawamura on the oikia of Minoo-B (1958) might be mentioned here.

In his studies, Kawai found it necessary to distinguish two kinds of dominance rank, "basic rank" and "dependent rank." "Basic rank" pertains to the situation where the dominant-subordinate relationship between two monkeys is recognized without interference from other monkeys, while "dependent rank" pertains to the situation where that relationship is recognized with more or less interference from other monkeys. Thus Kawai says (1958: 112):

From the developmental viewpoint, it is the dependent ranking that appears first. In the baby stage, these monkeys live in close association with their mothers, and are always under the protection of the latter. So they can behave after their mothers' superiority and gain dependent rank. On the other hand, they themselves are the lowest of the basic rank, or more precisely, they lack basic rank.

The basic rank becomes recognized when they have grown to be able to behave independently of its mother [sic] and claim to have their own living.

That is, given two infants and their respective mothers, the one whose mother is more dominant in basic rank manifests its dominance over the other one whose mother is less dominant in basic rank, even if the latter infant is more dominant than the former in terms of their own basic rank. Behavioral expression of such "dependent rank" is usual among the female and immature Japanese monkeys observed in the field. When such a dependent dominance status is fixed by social

conditioning, it may become a basic dominance status. The basic rank is, therefore, not always attributable to physical and/or physiological factors.

The basic rank of males in the oikia of Koshima in 1957 was as follows (each animal being signified by a number):

$$\begin{array}{ccccccc} & & 10 & > & 8 & & \\ \delta : 1 & > & 2 & > & 3 & \begin{array}{c} \diagup \\ \diagdown \end{array} & \begin{array}{c} 9 > 14 > 15 > 13 > 12 \\ 11 \end{array} \end{array}$$

This roughly represents the age order among them, except for 12, which seems to be feeble-minded. Among them, 8, 9, and 10 are five years old; 11 and 12 are four years old; 13 is three years old; and 14 and 15 are two years old.

The basic rank of females in the same year was as follows:

$$\begin{array}{ccccccc} \text{♀} : 101 & > & 105 & > & 108 & > & 111 & > & 109 & > & 110 & > \\ & & 104 & > & 103 & > & 107 & > & 113 & > & 112 \end{array}$$

Contrary to the case of males, this basic rank does not at all represent the age order of the females. Thus, 107 is six years old; 108, 109, and 110 are five years old; 111 is four years old; and 112 and 113 are one year old. The ages of males and females born before 1950 cannot be determined.

As in the oikia of Takasakiyama observed by Itani (1954), infant males in Koshima retire from the central part to the peripheral part of the oikia when they become juveniles. This phenomenon may be designated *peripherization*, after Kawamura (1958). Kawai has suggested that, correlating with this movement, the behavior concomitant with the dependent rank of the infant in the central part of the oikia decreases, while independent behavior based on the juvenile's own ability increases. As a result, the basic rank among such males is reorganized during and after peripherization. Kawai understands the above-outlined basic rank among young males 8, 10, and 11 as a manifestation of such in-process reorganization.

Females, in contrast, remain in the central part from birth to death, so that they constantly grow under the influence of dependent-rank effects, and this, in turn, induces among them a solidification of dependent rank into basic rank. Kawai attributes to such a social process the lack of correlation of the basic rank of females with their age order.

At Koshima, three of the adult males (1, 2, and 3) are dominant in basic rank over all the other monkeys including the females. However, in the real life of the oikia, where various dependent-rank effects always co-exist, such a simple relation as might be found between two animals out of the field of any dependent-rank effects does not usually appear. Kawai put a test box, about 50 x 50 x 60 cm. in size, in the feeding place, and observed in what order monkeys of the oikia mounted the box and ate the wheat on it. He called this order the "feeding rank," i.e., a single rank compounded of the basic and dependent ranks including both males and females, and sanctioned by all oikions under these

specific circumstances. The result is shown in Table 2.

In Table 2 (where numbers in the hundreds signify females), we can see from cases I and II that, under the influence of 1 ♂, 101 is privileged to be dominant over 2 ♂ and 3 ♂; likewise, 105, 108, 111, and 109 are privileged to be dominant over 3 ♂ in case I, but, when the influence of 1 ♂ is weakened in case II, that is, when he is situated far from the test box, then the dependent-rank effects on these four females are also weakened, and 3 ♂ recovers his basic dominance over these fe-

TABLE 2
FEEDING RANK IN THE OIKIA OF KOSHIMA in 1957

Case	1 ♂	2 ♂	Feeding Rank *
I	++	+	1, 101, 2, 105, 108, 111, 109, 3, 110 ...
II	+	+	1, 101, 2, 3, 105, 108, 111, 109, 110 ...
III	-	+	2, 101, 105, 108, 3, 111, 109, 110 ...
IV	-	+	2, 101, 3, 105, 108, 111, 109, 110 ...
V	-	-	3, 101, 105, 108, 111, 109, 110 ...

* Single-digit numbers represent male monkeys; numbers in the hundreds represent females.

** + and - indicate, respectively, the presence and absence of the animal.

males. Case III shows the recovery of 2 ♂ in the absence of 1 ♂. In this case, 105 and 108 are once again privileged in the presence of 2 ♂ to be dominant over 3 ♂, as they were privileged in the presence of 1 ♂ in case I, but 111 and 109 are no longer so privileged. If the influence of 2 ♂ is weakened, as in case IV, 3 ♂ recovers his dominance as in case II. Finally, in case V, where the influence of 1 ♂ and 2 ♂ is completely lacking, 3 ♂ manifests his basic dominance over all the females. Females situated under 109 in the basic rank are not influenced by the presence or absence of any of the three adult males. Females which are privileged by consort relationship have been carefully excluded from Table 2.

From these observations, then, Kawai has determined the social status of each male and female oikia relative to its fellow-oikions. I reproduce his findings as Figure 2, following the same generalized schema already given as Figure 1. This is also a revision of Kawamura's schema (1956a) of the spatial distribution of oikions in this same oikia of Koshima, which was based on data obtained by him in 1954.

KINSHIP RELATION

If a higher dependent rank is accorded the infant of a more dominant female, and if, when this infant is female, the dependent rank is easily transformed into basic rank, as Kawai claims, it is expectable that basic rank among females will not correlate with age order, as is the case among males, but that the daughter of a dominant female will occupy a high status next to her dominant mother. This tendency has already been exemplified with reference to the oikia of Koshima (see basic rank of females in 1957, above), where 108 and 111 are daughters of 105.

This tendency reveals itself fully, however, in the oikia of Minoo-B, which has been intensively studied since 1954. This oikia is peculiar in that it has no male leader, though two adult males have spontaneously

joined it from the neighboring oikia, Minoo-A. Kawamura says (1958: 149):

It is recognized that there are two important principles forming the rank system of the Minoo-B Group. The first of them is that an infant is ranked after its mother's rank, and the second is that, among brothers and sisters, the younger is ranked higher than the older. In a supposed group of pure matriarchy, these two principles are combined and involve a rank system as shown in Fig. 1.

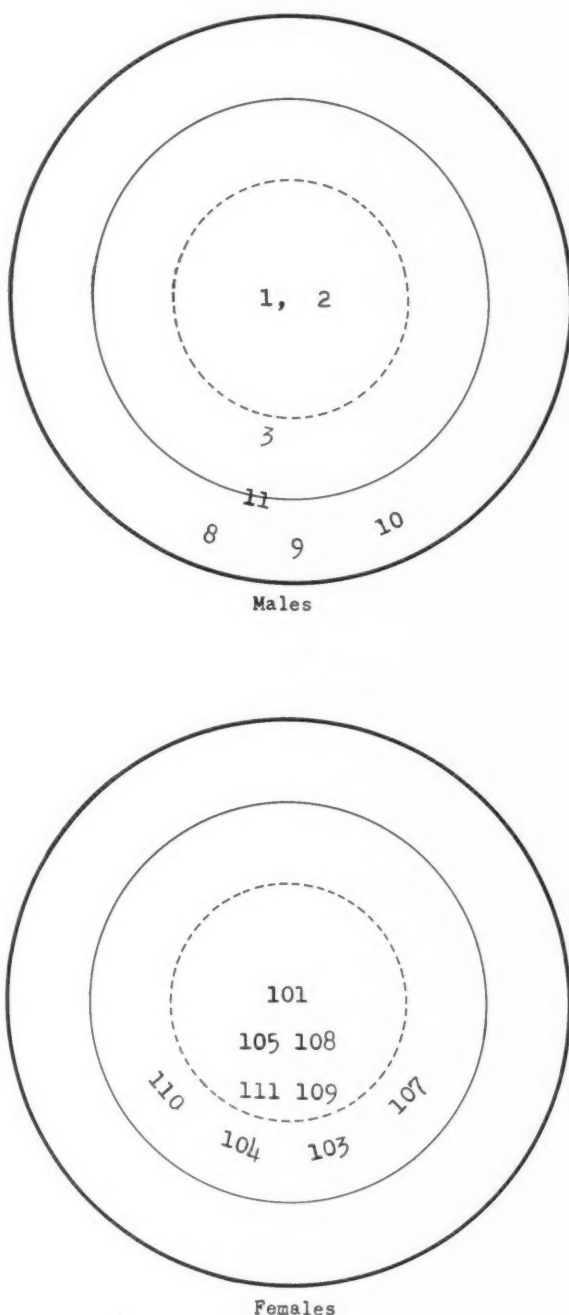


FIG. 2. Spatial distribution of oikions in the oikia of Koshima in 1957.

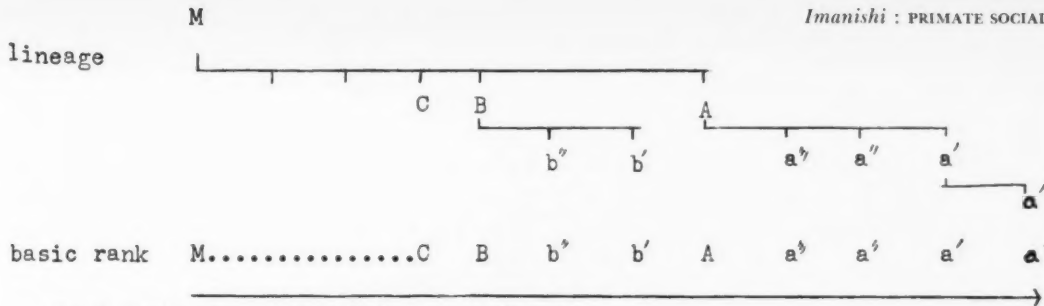


FIG. 3. Rank system of a hypothetical matriarchal and matrilineal oikia. M = mother; A, B, C, ... = daughters; a', a'', a''', b', and b'' = grand-daughters; a' = great grand-daughter. Ages of these individuals are $A > B > C$, $a' > a'' > a'''$, and $b' > b''$.

His schema of the rank system of a hypothetical matriarchal and matrilineal oikia is reproduced here (Fig. 3), together with the actual rank system found by him in the oikia of Minoo-B in 1958 (Fig. 4).

In Figure 4, oikions under three years of age have been omitted because they are so strongly under dependent-rank effect relative to their mothers' ranks that their basic rank cannot be adequately determined. Two young males shown in Kawamura's original report are also excluded from Figure 4 because they were already in process of peripherization in this matriarchal and matrilineal oikia. Figure 4 reveals one recognizable exception to the principle, in the relation between Buna and Nemu. Kawamura considers this to be due to the timid personality of Buna. In other oikiae, the younger daughter is not always ranked higher than the elder. In the oikia of Koshima, for instance, 108 is older but ranks higher than her sibling 111. Kawamura considers this to be due to dependent-rank effect relative to dominant males (e.g., see Table 2, case III).

Itani (1958), Kawamura (1956b), and Yamada (1957) have already pointed out that the process by which any oikion learns various habits and social relations is closely connected with its kinship position within the oikia. Imanishi (1957b) has suggested extending application of the concept "identification" from the socializing process among human primates to that among subhuman primates. That is, I have pointed out (1957b: 3) that:

Children of dominant females involuntarily learn attitudes of the dominant and those of submissive females learn attitudes of the submissive. Moreover, children of dominant females in the central portion are more intimately related to the leader than those of submissive females in the peripheral portion, as their mothers are more intimately related to the leader.

Therefore, children of dominant females may be able to identify more smoothly with their leaders. Imanishi has written also (1957a: 53):

If other things are equal, males growing up with successful identification will cooperate with leaders more willingly, be accepted by them as well as by females more easily, and finally succeed their leaders. . . .

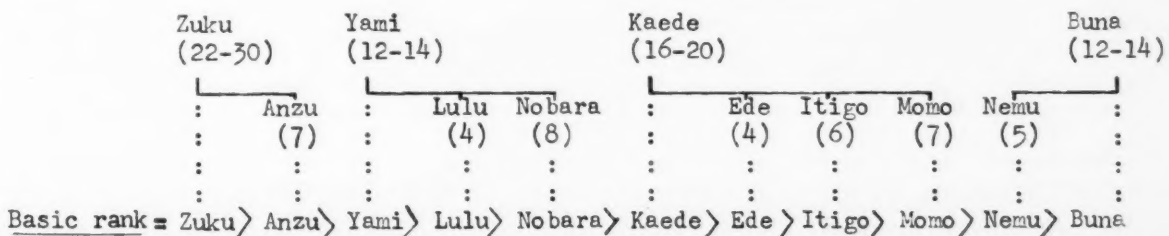
In the oikia of Shodoshima-K, a young male was found, in the central part, making no move whatever toward peripherization. According to Kawamura (1959), Yamada recently observed that this young male had won the status of leader. Though he was assumed to be the son of some dominant female, proof was still wanting. I am inclined to agree with Kawamura's anticipatory opinion that, if this is the explanation, it represents one of many different courses by which young males become leaders.

EPILOGUE

Although there are various fruitful approaches to the study of subhuman primates in their natural habitat—e.g., the demographical, the ecological, the ethological, and the psychobiological—I have in this paper focused on the sociology of Japanese monkeys, and, in particular, on that which has been learned about certain central problems in social organization from the intensive observation of identified individuals over comparatively long time-spans.

Except for the question of a breeding season, the resolution of which does not necessarily require the identification of individuals, no structural and functional elements of primate social organization can, in my opinion, be definitely demonstrated except by the method of individual identification. Since the findings in the cited works by Zuckerman, Lumsden, Buxton,

FIG. 4. Pedigree and basic rank of females in the oikia of Minoo-B, 1958. Number in parentheses indicates the age of the individual.



Haddow, and Donisthorpe were not a result of application of this method, the conclusions of these authors will invite future re-examination by the same method.

The study of Japanese monkeys has been carried on not only according to the method of individual identification, but also with exact knowledge of the pedigree of each individual. This latter has yielded insights into the importance of kinship ties in the social organization of subhuman primates, something hitherto little noticed. As this kind of study is continued, there may be found a way to solve the question of the origin of the incest taboo or of the family in human societies.

Further, though I have not mentioned the fact in this present paper, there are behavioral variations from oikia to oikia of the Japanese monkeys under observation, to the extent that any generalization on the basis

of observation of only one oikia cannot a priori be considered valid (Kawamura 1956b; Frisch 1959). Accordingly if an experimental oikia is created successfully from individuals of two different oikiae, it may in course of time reveal whether and how much behavioral characteristics derive from heredity and/or culture.

The fruitfulness of the provisionization technique has been proved to some degree in the study of Japanese monkeys. By now, twenty-one oikiae of Japanese monkeys in various parts of Japan are provisionized, and fourteen of them are under study by the method of individual identification. Above all, two of them are under the management of the Japan Monkey Center as experimental oikiae. Prospects of the study, so far as Japanese monkeys are concerned, are, I believe, very promising.

Comments

By FRANÇOIS BOURLIÈRE*

(1) Imanishi is perfectly right when he criticizes the use by too many field naturalists of terms such as "harem" and "clan," which have a very precise meaning in cultural anthropology. To date, most of the naturalistic studies of the social life of wild primates have been of extremely short duration, and none has been based on prolonged follow-up of free-ranging groups, the various members of which were individually known to the observer. That is precisely what makes the Japanese studies unique in the field of infra-human sociology.

(2) The observations recorded in this paper represent more instances of that intra-specific variability of social behaviour which seems almost as characteristic of primates as their morphological variability. The case of the Minoo-B group, which has no male leader, is particularly striking.

(3) The importance of age as a determinant of social hierarchy, at least among males, is also worthy of remark. May I point out, however, that this characteristic is not at all peculiar to primates, and not even to mammals? Reliable reports have demonstrated its existence in social birds: e.g., W. J. L. Sladen (1958) has described at length the role of the various age-groups in the complex society of Adélie penguins (*Pygoscelis adeliae*).

By C. R. CARPENTER*

Imanishi's splendid review of the results of field studies of nonhuman primates and especially of the very significant contributions of the Japan Monkey Center raises a number of major problems which primatologists

and anthropologists need to consider.

(1) *Inter-Cultural or Inter-Language Communication*. When concepts, abstract thought, and even observations are formulated in English, translated into Japanese, and re-translated into English, both the denotive and connotative qualitatifs of terminology and language are changed. The largely qualitative descriptions now of necessity used by field observers of free-ranging primates are often originally inadequate to reflect accurately the observed behavior. Translation and re-translation of such descriptions increases the difficulty of arriving at consensuses about the common-denominator meanings, first of observations, and then of the language vehicles.

At this stage of development, or lack of development, in the branch of science that requires the investigation of organized populations of nonhuman primates in their natural habitats, observers are confronted with the exceedingly difficult task of contributing to the emergence of terminology and language, graphic and pictorial signs and symbols, and mathematical expressions which are true to the observed data and with respect to which there can be inter-cultural and interlingual agreement. Clearly, there is a need to progress from qualitative descriptions, though without neglecting the refinement of them, to the formulation of quantitative expressions which accurately represent behavior, social interactions, characteristics of group organizations, population dynamics, and ecological factors. Field studies of primates, including man, will eventually require appropriate field theory and field mathematics. A developed, known, and accepted language of mathematics will reduce the distortions and ambiguities now involved in translations of verbal descriptions of observed phenomena.

There is another important aspect of inter-cultural scientific communication. There are differences in traditions of criticism, so that (for example) Oriental scientists may think improper the skepticism and challenging criticism that Western scientists consider indispensable in a developing branch of science. Such differences may inhibit free criticism in both directions.

Finally, on this problem, the increasingly numerous primatologists of all nationalities interested in the naturalistic studies of nonhuman primates demonstrate extreme variability in training, in approach to research, in sophistication in biological and behavioral science theories, and, accordingly, in the technical language that they command and use. The requirements of objectivity and accuracy are often violated by the use of inferential, subjective, and anthropomorphic language. Field primatologists are at present a heterogeneous interdisciplinary group representing psychology, anthropology, the medical sciences and arts, biology, and ecology. Thus, to the problems of inter-language and inter-cultural communication are added the problems of interdisciplinary communication.

(2) *The Place of Theory in Naturalistic Research on Nonhuman Primates*. This difficult problem is raised early in Imanishi's review. Primatologists generally assume theories of evolution. Likewise, they accept genetic theories. Most of them are aware of theories of ecology like that of biotic equilibria and dynamics. All use some form of behavioral theory including theories of development, motivation, and learning. Therefore, field studies which do not expound theory may nevertheless be oriented both from and to theoretical systems or sub-systems of principles, propositions, and hypotheses which provide a theoretical

frame of reference for the investigators.

The following questions are raised by Imanishi's review: How is adequate theory derived? When in the sequences of scientific thought should theory be the focus of attention and effort? What should be the order of theoretical formulations relative to observations? And, finally, what is good theory, and what is its effect on the quality and productivity of research, especially of field research?

Imanishi seems to propose that individual identification of animals, "provisionization," "acculturation," the "oikia" concept, "basic rank," "dependent rank," the breeding season, and "peripheralization" constitute needed advances in theory. It is suggested that individual identification and "provisionization" are important techniques or procedures, and are not theories. The other terms mentioned above are highly general ones which reflect ways of describing the results of extensive observations made over time. General theory and dependable principles are probably just emerging in the research reports of field primatologists. Furthermore, theories (if they be such) like "acculturation," dominance ranks and statuses or classes, kinship behavior, etc., are transposed from other disciplines. The substitution of new terms for previously used terms should perhaps be carried out with care and caution and justified by evidence. There is a fine but necessary distinction between the developing of a new, useful, and adequate scientific terminology and the using of neologisms. Single or limited terms, new or old, do not constitute adequate theoretical statements. Words like "harem," "group," "troop," "cluster," "sub-group," or "oikia" are not statements of theory, but, rather, highly generalized symbols. Theory is a generalized, unproven and tentative statement, or series of statements, based on sets or systems of observations that orient the collecting of additional observations and evidence, which, in turn, when collected, systematized, and interpreted, may modify the earlier statement of theory. General theory can be used to increase the accuracy of predictions of a range of related but specific behavior, interactions, and social-organization characteristics.

Some theories, when used in the course of a scientific attempt to build a body of dependable knowledge, e.g., on the social behavior of free-ranging primates, may limit and restrict observations. Assumptions may operate similarly. This focusing of observations may be advantageous, for instance in proving an hypothesis, or, the focusing may prevent the observer from seeing,

appropriately evaluating, and recording behavioral events that are not within the range of observations focused by theory.

Imanishi is certainly correct in calling for theory construction—the developing of field methodology—and in contributing to this effort. But there are steps, developments, and sequences of events, such as earlier field studies and even present-day field studies, which may be necessary preliminaries to the formulation of adequate theories. Furthermore, as has been stated earlier, naturalistic studies or contextual research on primates, nonhuman or human, reflect some theory, however general or specific. In addition, it might be observed that clearly-formulated questions like, "What is the composition of groups in terms of central tendencies and variabilities of a species of primate?" may serve well to guide field observations. By contrast, hypotheses like those which postulate that nonhuman primates are monogamous, polygamous, or promiscuous may bias, distort, or even preclude accurate observations. An approach of seeking answers to hundreds of simple direct questions about all characteristics of howler monkeys' behavior, and social and ecological interactions, seems to have been relatively productive while not primarily theoretical.

Perhaps what the emerging area of field studies of nonhuman (and human) primates now needs is a complete system of categories of questions that need to be answered. The answers could then be collated in defined categories for all species of nonhuman primates. The resulting information pool would correspond to the Area Files of anthropologists. This approach would contribute importantly to the rigorous formulation of testable hypotheses and the construction of general theories. Surely, theoretical hypotheses do sharpen and intensify observations.

(3) *Generalization of Observations across Genera and Species.* Imanishi's review has raised this problem in sharp definition. The central task here, as Imanishi clearly realizes, is that of describing, charting, and measuring, when possible, the full ranges of characteristics of nonhuman primate behavior, including social organization and grouping patterns. This requires study of the variabilities which occur in different genera and species. Variabilities are to be expected in social behavior and organization, both in the higher-order complexes of behavior where learning has a prominent role, and in behavior which is anatomically

determined and limited like postures, prehension, locomotion, and sound production. Such study must take account of habitat and environmental determinants of the observed similarities and differences. Currently available data, collected on an inadequate sampling of genera and species by a heterogeneous group of observers, and using different methods in different environments, do not provide the necessary information for completely adequate comparisons over the full range of the sub-order of nonhuman primates. In the meantime, we must do as Imanishi has done, make comparisons among the species for which we have relatively reliable and fairly adequate information. Much long-term and intensive field work is needed on many little-known species to complete the picture. Our conclusions, therefore, must be tentative, and variations, gaps, and inconsistencies in our knowledge are to be expected.

The length of this commentary precludes a detailed criticism of Imanishi's very stimulating review. Most of the detailed differences in language, viewpoints, inferences, and opinions could be resolved best by co-operative simultaneous field work in the same context with the same species of primate. When the objects of observation are confronted by several observers, it may be possible to align concepts and develop common understanding.

Imanishi and his colleagues at the Japan Monkey Center and elsewhere throughout the world where they are conducting field studies, deserve to be highly complimented on their effective efforts to add to the knowledge in primatology. For thirty years field studies of monkeys and apes have been the lonely fate of a few hybrid scientists. Now, upsurges of interest and effort are occurring, and men like Imanishi are importantly responsible for this rapid development in naturalistic and contextual research on the nonhuman primates.

By M. R. A. CHANCE*

Imanishi's paper marks a distinct advance in our thought about the social organization of sub-human primates, for by collating the various reports on different species of social primate in the wild it adduces new evidence on the role of sexuality in primate societies.

While Zuckerman never explicitly stated that he considered the factor underlying the permanent association of the sexes was persistent sexual at-

traction, this has, in fact, been considered to be one of the main purports of his work.

Elsewhere Mead and I have shown that *long periods* of sexual receptivity in the female primate lead to intense conflict (not necessarily overt) among males, as can be deduced from the nature of reproductive life in other mammals (Chance and Mead 1953). In most species of mammal, pairs separate for breeding from other pairs, and thus disrupt what, in some instances, is an otherwise continuous social group. Thus in other mammals sexuality is a disruptive element, and, therefore, an added bond is required to explain the sociability of monkeys. While Imanishi has not directed his attention to this—a point which it will be most interesting to have his comments on in the future—still, by showing that many species of monkeys have a cyclical breeding activity without interruption of their sociability, he has emphasized that sexual attraction cannot be the primary bond of primate society.

To discover that sexual attraction is, not a bond, but a disruptive element, in a society of monkeys or apes, is not necessarily to contradict the statement that the factors underlying associations of monkeys and apes are connected with the prominent part that sexual behaviour plays in their social repertoire—a point Zuckerman draws attention to separately, but which later became treated as synonymous with continuous reproduction (which he deduced from other evidence by shooting the wild troop of baboons). These are two quite different features of the primate, as sexuality is an attribute of behaviour, whereas reproduction is a biological attribute and one which is not directly correlated with sexual performance.

It is less easy to be wholeheartedly in agreement with Imanishi's second disagreement with what Zuckerman wrote, about the structure of primate families. To my mind the term "family" is no more than a casually descriptive one when applied to primate societies, and is yet another example of terms like "neurotic" or "convulsive" which, transported from their human context, carry implications which are later found not to reside in the nature of sub-human vertebrate behaviour. Hence, I consider that this discussion is somewhat less substantial and revolves round the use of a word which is itself not an accurate theoretical concept. Similarly, although I think the word "oikia" has more to recommend it, in the way it is defined by Imanishi, than the word "family," it tends to introduce a classification which may rigidify. While it at present

enables us to classify and distinguish the different forms of the social unit, it may deflect attention from the fact that these units are not necessarily characteristic of a species but of a species living under certain conditions. This has already been demonstrated by the work of Collias and Southwick (1952). The essential characteristic of a primate society is the fact that members of both sexes live together within the structure created by a male dominance order which may cause a break-up into "family" groups, or groups with other special characteristics.

Thus, I would direct more attention to the dynamic characteristics of these groups, in an attempt to define those underlying processes of an individual's behaviour which are responsible for its interactions with other members of the group.

That there is more than one type of bond seems evident, just as there is more than one type of dominance relationship. Here Imanishi has given us both a useful distinction, in bringing forward evidence that dominance can be based upon hereditary factors or upon acquired patterns of behaviour, and has also, I think, added clarity to this field in showing how these two can interact.

By JOHN T. EMLEN, JR.☆

The inadequacy of channels of international communication, the problem to which CURRENT ANTHROPOLOGY has dedicated its energies, is responsible for a wide ignorance of the important contributions which a group of primatologists in Japan have been making towards an understanding of the basic mechanisms of social organization, in higher animals. Imanishi has rendered a great service by bringing together for a world audience some of the important findings of the members of his Institute. Of particular value is his review of observations on kinship relations and their role in the determination of social rank in free-ranging populations of the Japanese Macaque. Through persistent and patient watching of animals and their social interactions over a succession of years, Imanishi and his colleagues have demonstrated that the social ranking of individuals, especially females, is often determined indirectly by association. The situation closely resembles certain well-known phenomena in human socialization, and Imanishi cautiously though convincingly suggests that the social psychologists' concept of "identification" may legitimately be applied to the development of these simian behaviors.

The problem of semantics often arises in studies of non-human behavior, especially where strict objectivity demands the avoidance of words which hold, or seem to hold, human implications. The words "group," "subgroup," "troop," "family," and "harem" have given rise to confusion in studies of primate societies, and Imanishi has proposed that two new words, *oikia* and *oikion*, be introduced to dispel this confusion, the former to designate the minimum unit of social life regardless of composition, the latter to designate one of the components of an *oikia*. Such new terms are, to be sure, free of undesirable implications. I question, however, whether workers will not still find it necessary to define terms as they apply them to different species and different situations. In comparative studies I doubt that any adoption or revision of terminology will solve our basic problems of understanding the evolutionary significance of similarities and differences. What is needed is an operational approach with a minimum of classified technical terms.

While on the subject of terms, I would like to take this opportunity to join Allee and Carpenter in protesting the widespread and indiscriminate use of the words "subhuman" and "infrahuman" in situations other than those which purport to compare the evolutionary progress or performance levels of various animals with those attained by man. These terms reflect an anthropocentric viewpoint which is generally unintentional but which should be consciously avoided by anthropologists and biologists in the interest of over-all objectivity of approach. Where the issue is simply one of excluding man from consideration, the word "non-human" would seem to be preferable.

The comparative study of social organization in primates has been a fertile and fascinating area for speculation. With only a handful of species examined, even superficially, we are already besieged with a flood of challenging theories and suggestions on underlying social principles, origins of human social behavior, and even origins of higher mental capacities. Such speculations play a valuable role in stimulating and guiding new research, but the real need at this stage is clearly for more solid facts of the kind that the Japan Monkey Center has been accumulating. Intensive studies must be made on representative members of all major groups to reveal, not only the nature of differences between taxa, but the range of variation occurring among ecologically separated populations of single species. Comparative studies of a wide variety of vertebrate and even

invertebrate animals are, in fact, needed to provide the best possible perspective for interpreting the complex pattern of converging and diverging trends which characterize the evolution of behavior in any single group. It may be worth noting, for instance, that the closest parallels with man in the area of family development—sharing of parental duties by mated pairs, and even connubial fidelity—are to be found, not among non-human primates, but among birds. As in any comparative study, the basis for detecting homologies, or for tracing evolutionary trends, can best be found through a wide acquaintance with organic variation.

By ADOLPH H. SCHULTZ*

(1) The discovery of such a surprisingly clear-cut breeding season in the Japanese Macaque presents problems for specialists in the physiology of reproduction as well as for students of social behavior. Such an extreme seasonal limitation of reproduction is rare in the suborder of simian primates, among which a more or less recognizable *preferential* breeding season seems to represent the rule. At any rate, for the majority of wild-shot tropical monkeys and apes, collectors have obtained all stages of pre- and post-natal development at all times of the year. In howler and in spider monkeys, for example, I found early embryos, fetuses of varied sizes, and nursing infants, all at practically the same time and place. By establishing a colony of the exceptional *Macaca fuscata* at some tropical station, one might detect possible environmental factors influencing such seasonal changes in the birth rate.

(2) In the interesting discussion of dominance rank, it is stated that this is "not always attributable to physical and/or physiological factors," but, of the latter, only some data on age are given. It would be interesting to correlate rank also with the relative size, sexual status, and, whenever possible, with the health, of the individuals. Heavy parasitic infections, miscarriages, etc., are far from rare in wild monkeys, and alveolar abscesses, arthritis, healed fractures, etc., become very frequent with advancing age and are bound to influence ranking behavior. That "leaders of the oikia are estimated to be over twenty-five years old" (footnote 3) is hardly tenable, in view of the fact that skeletons of captive monkeys, known to have lived over twenty years, all show unmistakable signs of senility, with bony atrophy, very advanced wear and loss of teeth, and usually multiple arthritic changes.

(3) According to the welcome data

in footnote 1, of 100 to 115 mature females, 59 had offspring between May and November, and the others had apparently not conceived (due to lactation or old age?). This high birth rate would be expected to surpass the death rate in a monkey population, such as this in Japan, which is not held in check by predators. These newborns, as well as the 120 infants, contain practically equal numbers of both sexes; but, among the older individuals, there are only 66 males as against 135 females, indicating a much higher mortality in the former. It is to be hoped that Imanishi and his collaborators will investigate the reasons for this differential mortality rate.

(4) Most non-primatologists take it subconsciously for granted that man has climbed to the highest branch on the primate family-tree while all our contemporary simian relations have remained below us in every respect and hence can simply be called "subhuman." However, there are a great many features with respect to which man has not become as highly specialized as have some other primates, but, rather, approximates the original condition more closely. The popular term "subhuman" implies a scale of evaluation which can be used merely for single characters and becomes a misleading generalization when applied indiscriminately to all *non-human* primates.

By S. L. WASHBURN and IRVEN DEVORE*

Our observations on baboons in Kenya indicate that there is a marked seasonal peak in births at the beginning of the rains in October. The female has a clearly-defined sexual cycle, and is receptive for less than a third of this cycle. Early in estrus, females solicit the attention of the males, and during the later phases the males initiate sexual activity and form temporary "consort relationships." Troops number from 9 to 185, and within the troop there are no subgroupings which could be called "families" or "harems." The minimal social unit is a female and her infant. Subgroups consisting of an adult male and one or two females, plus infants or juveniles, are formed frequently. These are temporary preference groups, and the estrus female remains in no such subgrouping. In summary, sexual activity in Kenya baboons is marked by: (1) a seasonal birth peak, (2) female estrus, and (3) promiscuity, modified by temporary male-female consort relationships.

On the basis of far less evidence, we think that the situation in vervets (*Cercopithecus aethiops*) is similar. There is a birth peak and no evidence of "family" or "harem." Dominance is present but less marked than in baboons. In both baboons and vervets the only stable, lasting social unit is the troop. The troop exists because it helps the members to survive in many ways. That sexual attraction is an entirely inadequate explanation for the troop is best shown by the fact that during most of her life a female is not involved in sexual activity. Most of the time she is either juvenile, out of estrus, pregnant, or lactating. Yet she does not leave the troop even for a few minutes. She is most central in the structure of the troop when she has a new baby, and is often peripheral when she is in estrus.

We are doubtful that it will be useful to anthropologists to have a single word ("oikia") for all kinds of minimal social units. Terminology should not obscure the fact that a gibbon group of male-female plus young, a baboon female and infant, or a troop, are fundamentally very different units.

The continued study of provisioned monkeys offers the opportunity of analyzing many problems. The great contribution of Imanishi and his co-workers is the understanding of the social structure which comes from continued study over the years. The stability of the hierarchy, age of the leaders, the relation of female dominance to the position of the young, sibling relations—in these our only data on free-ranging primates comes from the Japan Monkey Center, and we hope that more of their observations will be published in CURRENT ANTHROPOLOGY.

By SIR SOLLIE ZUCKERMAN*

Imanishi's most interesting review, which summarizes some striking work that has recently been carried out in Japan, has also been framed to give the impression that the information about Primate sociology collected since 1932 necessarily controverts certain basic principles which had already emerged from observations of captive and wild baboons. A careful study of the new material makes me doubtful whether these new data are, in fact, inconsistent with the general theses first developed in my *Social Life of Monkeys and Apes*, to which Imanishi refers so generously.

The first of these general theses was that the continued association of the adult members of the basic social units

("family parties") depends upon factors connected with the recurring menstrual cycle characteristic of all Old World Primates. Imanishi appears to be under the impression that this implies a uniform rate of breeding throughout the year, and in an attempt partially to question the thesis, quotes data which appear to indicate that there is a seasonal increase in the numbers of births in certain Primate species. At the time my book appeared I was aware that seasonal variations occur in the incidence of births in various species of monkey, and have, in fact, written two or three papers on the subject. Nonetheless, if it eventually emerges that in some higher Primates the numbers of births fluctuate from month to month, this would not necessarily be of significance, as, for example, the variation may be due to factors other than changes in the sexual cycles of the females. Even the possibility that there may be seasonal variation in the intensity of sexuality does not affect the basic issue, that there is always a sexual-social link. The new data relating to an apparent seasonal increase in the frequency of copulation in the Japanese Macaque are interesting, but as presented in the review it is not possible to assess whether or not they represent a true fluctuation or whether the monthly variation can be reasonably attributed to chance.

The second basic principle of Primate sociology, to which Imanishi refers, is that the composition of the social unit together with the behaviour of its members depends on the position of the principal male in a scale of dominance relative both to other members of his own unit and to those of neighbouring groups. This proposition gains support from Carpenter's studies of the social organisation of rhesus monkeys, and although initially it appeared that an island population of howler monkeys was organised differently, it is noteworthy that in the course of some twenty years the composition of its units changed to resemble that of the family parties of Old World monkeys.

There is little doubt that some variation exists in the composition of the basic social units, but provided these are homologous in different species there is an obvious advantage in Imanishi's suggestion that a single standard term should be used for their description. Such variations as exist may be correlated with differences in the manifestation of dominance among the males, and Imanishi's techniques for studying individual groups from free-ranging colonies have already provided significant new data relating to this aspect of the problem.

Reply

By KINJI IMANISHI

Bourlière, asserting the "importance of age as a determinant of social hierarchy, at least among males," points out the importance of age in the social organization of Adelie penguins. I applaud his emphasis, for I have regarded sex and age as two of the principal factors segmenting societies of higher animals. Among male Japanese monkeys, relative age seems to be a more basic and stable principle of organization than dominance rank, which is complicated by status differences among individuals belonging to the same age class. As Schultz rightly remarks, dominance rank is apt to be influenced by individual peculiarities such as state of health, etc. It is recognized that dominance rank among young, peripheral males is highly unstable. However, once a male becomes a central male or leader, he never reverts to being a peripheral male because central males and peripheral males form two distinct age classes. Nor does an aged leader always rank low. The former head leader of the oikia of Shodoshima S was so old that he could not climb a tree any more; notwithstanding, he retained his status as leader and fulfilled this role until he died in his oikia (observed by S. Kawamura). As a supplementary piece of information: a leader of the oikia of Takasakyama disappeared when he found he could not keep his status as a leader (Mizuhara 1957: 186-203). Schultz infers from the numerical discrepancy between post-infancy males and females in the Takasakyama oikia that the males suffer a much higher mortality. This may be true to some extent, but it is also true that, of the considerable number of solitary males, some have left the natal oikia in the process of peripherization.

Both Schultz and Emlen protest the use of the word "subhuman." I also would not apply the term indiscriminately to all "non-human" primates, e.g., to lemurs or bushbabies. But as a social anthropologist I am interested in the origin of culture, and, in my usage, the "subhuman" level of primates is nearly equivalent to the "subcultural" level of primates.

It is well known that there are two distinct but complementary scientific approaches, viz., the nomothetic and idiographic. While Carpenter seems to recommend the former approach, I am rather inclined to pursue the latter in the long-term observation of Japanese monkeys. However, as Carpenter says, it is indeed necessary for us to find a way to overcome the difficulties of inter-

cultural or inter-language communication and reach mutual understanding in the common field of study.

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Publications Received

The Editor wishes to acknowledge with thanks the receipt of the publications listed below.

To make our listings more accurate and serviceable, each publication sent to the Editor should include on the cover or title page complete information (with date, city, etc.), translated into English, and, where necessary, transliterated into roman script.

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Assyriology— Why and How?

by A. Leo Oppenheim

It is now well over a hundred years since Western European scholars succeeded in discovering the key to the writings that two long-vanished Near Eastern civilizations left behind. These writings are the hieroglyphic inscriptions on Egyptian buildings and objects, and the inscriptions, composed of cuneiform elements, on clay tablets and on stone and metal objects found in and around today's Iraq.

Ancient Egypt was always a strange, a curious country, exciting much interest and fascination in the minds of its neighbors. For nearly two millennia after its disappearance as a political and cultural force (under the Ptolemies, 342 B.C.), the inscribed and decorated walls of the unique and impressive ruins in the Nile Valley were successful in keeping alive some memory of the ancient Egyptian civilization. When then the fantastic Egyptian adventure of Napoleon and the quick decipherment of the Rosetta Stone by Champollion threw

open the buried civilization of Egypt and its ancient sites to the inquisitive eyes of European scholarship, a new world of undreamt-of complexity emerged. The historic vista of man and his adventures was "pushed back" by many centuries beyond the point reached by the Classical sources and the Old Testament.

Mesopotamia, however, the land between the two rivers, the Euphrates and the Tigris, was not nearly as fortunate as Egypt. There were no walls inscribed with mysterious and beautifully executed signs, hardly any precious objects to be collected as curiosities, nothing but a few high, isolated, and dilapidated brick towers to which clung the name and the fame of the Biblical Tower of Babel.

Only by the towering stone columns of Persepolis in the highlands of southern Iran, could the attention of European travelers be eventually attracted; inscriptions in an unknown writing found there excited their interest, and this led eventually to the decipherment of the cuneiform script that appeared on these stones and also on the large rock inscriptions of that region. Among these texts were inscriptions in a hitherto unknown language, and many more such documents were soon found all over Mesopotamia and the adjacent regions. The decipherers called the language "Assyrian." After a time it became evident that there was an Assyrian and a Babylonian dialect—we now refer to both as "Akkadian"—but the name "Assyriology" was retained for the field of study that deals with that language and its numerous dialects, all written with cuneiform signs on clay, stone, or metal.

In the heroic period of the science of Assyriology, which lasted until the last quarter of the nineteenth century, the various systems of writing using cuneiform signs were deciphered, the main content of the inscriptions was established, and the spades of the busily competing excavators attacked many of the principal sites, which began to yield objects of copper, silver, and gold, statues and fascinating reliefs, as well as the remnants of large-scale architecture. Since then, an abundant and steady stream of documents inscribed on clay has been coming to light everywhere from the Persian Gulf to Asia Minor and even as far off as Cyprus and Egypt.

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OPPENHEIM's publications range from Sumerian administrative documents to Assyrian dream-books. His interests comprise the history of technology, literary criticism referring to cuneiform texts, and problems connected with the history of ideas. The present article is part of the introductory chapter of a book on Mesopotamian civilization, which is still without definite title.

OPPENHEIM's paper was submitted to *CURRENT ANTHROPOLOGY* on July 14, 1959, and between October, 1959, and January, 1960, was sent for CA☆ treatment to twelve scholars, of whom the following responded with comments: William F. Albright, George Cameron, Jean Nougayrol, and E. A. Speiser. Some of the suggestions were utilized by the author. In late March, 1960, the revised manuscript was again sent out for CA☆ treatment, to the four earlier commentators, as well as to numerous others from whom comments had not been solicited in the first instance. Responses in the form of written comments were received from Robert Adams, Jean Nougayrol, and J. B. Pritchard. Oppenheim read these comments—both that of Nougayrol who for want of time did not prepare a publishable comment, and those of the two others whose comments are reproduced in full at the end of the paper—and decided they should stand without reply from him.

The record of achievement is impressive indeed. The decipherment led to the development of a series of new disciplines concerned with the study of the civilizations that either had made use of one or more of the several systems of writing or have become known through them. Here one has to mention Sumerology, Hittitology, and Elamitology, and to point out the study of the Hurrian and the Urartean languages as well as that of the remnants of the languages of early Asia Minor. Essential contributions were made by all these disciplines towards the understanding of the background and the surrounding world of the Mycenaean, Old Testament, and even the Egyptian civilizations. Finally, new vistas were opened up by the archaeology of the ancient Near and Middle East, which owes much of its success to the stimulus of the study of the textual materials.

In Assyriology proper, to return to the focus of this presentation, the textual evidence surpasses in meaningful relevance that of the monuments discovered, although the latter, especially the famous reliefs on the walls of the Assyrian palaces and the countless products of the glyptic art, often offer welcome illustration to the wealth of factual information contained on clay tablets, stelae, and votive offerings. The archaeologist's contribution towards the elucidation of the Mesopotamian past bears primarily on that crucial millennium or more which precedes the earliest written documentation (i.e., before 2800 B.C.), and which only the field and comparative archaeologists are able to scan and to articulate through their intricate network of horizons and sequences. In exceptional instances, however, and in small sites, the interplay of the archaeologist and the epigrapher can yield in Mesopotamia important results.

The cuneiform texts have given us a strangely distorted picture of the more than two thousand years of Mesopotamian civilizations. It is composed of a complex medley of abundant but very spotty detail information, and of rough and incomplete outlines of the major political and cultural developments. All that is torn to shreds by immense accidental gaps in time and space. It requires much patient and mostly rather hazardous work on the part of the philologist to hold these shreds together by a criss-crossing web of connections based on slim textual evidence. He has to link minutiae to minutiae, to analyze and to correlate a highly reluctant material in order to gauge developments and to trace their trends through the ever-recurring blackouts of information.

Thus we have come to know the names of hundreds of kings and important personalities from the third-millennium rulers of Lagash to the kings and scholars of the Seleucid period; we are able to follow the fate of dynasties and the personal fortunes of certain rulers, to observe the rise and decay of cities, and discern, at times, the settings of the geopolitical situation within a chronological framework that is becoming more and more reliable even for the earlier periods. We now have at hand a number of codified laws from the Sumerian to the Neo-Babylonian period that can be related to a staggering amount of private and public legal documents and illustrated by an equally extensive body of letters and administrative texts. This, in turn, has enabled the Assyriologist to realize period and local dif-

ferences, to observe changing social and political contexts, and has provided him thus with new and unexpected opportunities. No other early civilization offers material on its economic history with such abundance and for such a long period of time. Then there has been preserved a considerable body of texts that are customarily labelled literary. We have one full-length creation story and a bevy of shorter ones, the rightly famous Epic of Gilgamesh in a late and very sophisticated version together with a number of earlier fragments from all over in and around Mesopotamia, and several tales about gods and heroes of divine extraction, their exploits, triumphs, and sufferings often, but not always, harking back to earlier, Sumerian, prototypes. Their alluring contents, and the obvious relationship of these stories with the thematic inventory and even with specific incidents of myths known from neighboring civilizations, have given special importance to these texts in the eyes of the Assyriologists and in those of the scholars concerned with these civilizations. These texts have evoked far more interest than the literary texts of religious content, such as the numerous prayers, conjurations, lamentations, etc. Still farther in the background of the attention of Assyriologists and outsiders alike remains the immense bulk of the learned literature in cuneiforms. It consists primarily of the writings of several types of diviners, the handbooks of the Mesopotamian scholars ranging from Sumero-Akkadian dictionaries to learned commentaries and theological speculations. Only a handful of Assyriologists has ventured into these realms, dry, monotonous, and difficult of access as they are.

Assyriology is definitely an arcane discipline. Behind a facade of painfully inadequate popularizing presentations written for the interested but innocent outsider, a small group of courageous workers labors in an ever-enlarging field of research. Either in self-imposed concentration on a specific section or direction of approach, or compelled into such restrictions by the sheer bulk of the available data, these few scholars have been at work now for nearly a century. Under such circumstances, one may well ask the question as to where we stand today in the process of interpreting, correlating, and digesting textual evidence, archaeological findings, and monuments. Can we determine in some way whether the work that has been going on for such a long time in the universities of Europe, America, and Asia made adequate use of that unrepeatable intellectual experience which fate offered Western scholarship through all these inscriptions?

To answer this, I would like to establish here what these tablets meant to those who wrote them, and not assign them importance, meaning, and literary qualities derived, consciously or not, from our own culture-conditioned preferences. And then there is another problem: what can these tablets possibly mean to us of a late and alien civilization to whom they were not meant to speak?

What tablets we have from Mesopotamia can be easily divided into two main categories. There is, first, the large number of texts belonging to what I would like to term here "the stream of tradition": they represent what can for convenience be called the corpus of literary works of various types that was maintained,

controlled, and carefully kept alive by a tradition served by successive generations of learned and well-trained scribes. Then, second, we have the mass of texts of all descriptions united by the fact that they either recorded the day-to-day activities of individuals, whether shepherds or administrators, merchants or diplomats, or reported on such activities to some authority, whether priest, king, or—god.

Both these streams run, of course, side by side, with only limited but essential contacts: the texts of the second group could never have been written without that cultural continuum maintained so effectively by the tradition.

In the texts of the stream of tradition we have a body of literature which a class of scribes, organized in some loose way in local schools or in more or less fictitious families, considered its duty to copy and to recopy faithfully, thus keeping the chain functioning for nearly two millennia. This concept in itself represents an important culture trait of Mesopotamian civilization. One would expect the driving impulse for such a persistent attitude to be the desire to preserve a body of religious writings, or the wish to sustain one tradition against the opposition of, or in competition with, rival traditions. However, in Mesopotamia we are confronted, not with ideological pressures, but with a purely operational and highly effective device: it was considered an essential part of the training of each scribe for him to copy faithfully the texts that made up the stream of the tradition. The longer and more elaborate the training of a scribe was, the more extensive became the copying work he was supposed to do. This led quite naturally to the accumulation of a large number of private collections of tablets, each containing larger or smaller sections of the text material that made up the stream of tradition. Personal preferences, or the requirements of the training, contributed towards the development of private accumulations of topical composition. There even seems to have existed the tendency to obtain missing texts from outside collections in order to enlarge the material on some one topic available to a specific group of scribes. In this way, a number of scribes, widely scattered throughout Babylonia and Assyria, became owners of these literary texts which they had copied themselves during their apprenticeship or out of personal interest. Consequently, copies of the very same texts were kept in many different localities, and this, combined with the fact that the writing was on extremely durable clay tablets, maintained the major bulk of the texts from the second half of the second millennium B.C. up to the periods of the Seleucid and even the Arsacid (Iranian) rulers of Mesopotamia as a literary corpus in actual use, and subsequently kept them safe for us in the rubble of destroyed cities for two more millennia.

Most likely it will forever remain a moot question to what extent the corpus of texts under discussion remained unaffected by changes during such an extended period of continuous transmission. Have certain texts intentionally been discarded, or have others succumbed by a variety of accidents to the ravages of time and men? We know that all major and minor Mesopotamian cities were repeatedly and often quite effectively destroyed by enemy action, also that the water table has

been rising in lower Mesopotamia and that a sizable number of old cities are still inhabited today and therefore inaccessible to the spade of the archaeologist. These potential and actual losses are counteracted to a certain extent by lucky accidents: clay tablets are known to have been used as fill and entire archives to have thus been preserved, while certain sites happened to remain undisturbed when both victor and vanquished left the ruins to be forgotten and covered by dust and vegetation. While we must realize that we are to a large extent at the mercy of chance, we still have the duty to evaluate the possibility that certain selective manipulations may have interfered with the handing down of the traditional texts, or that new material may have been incorporated.

This problem is extremely difficult, and no clear-cut solution should be expected. There exists, however, the definite possibility of approaching it in a rather promising way.

It so happens that the last great Assyrian king, Assurbanipal (669–626 B.C.), succeeded in assembling in Nineveh what has every right to be called the first systematically collected library in the ancient Near



Tablet No. 4 of a Sumerian-Akkadian dictionary (usually referred to, by its first entry, as *e a = nāqu*) in a copy found in southern Babylonia and dating from the Seleucid period (about the second century B.C.). The work itself was probably composed in the first half of the second millennium B.C.

East. Large sections of the tablets that made up this collection are now kept in the British Museum, London. Many of them are published or reasonably well catalogued, and we have a good idea about the contents of nearly all these texts. Although the library was not that of an individual scribe or even a school or family, but, rather, was brought together upon a royal fiat from all over Mesopotamia, we are entitled to assume that the topical range of Assurbanipal's collection is representative of the main body, if not the entire content, of the scribal tradition. This assumption is borne out by a small but sufficient number of preserved private tablet collections that come from such widely scattered cities as Assur and Harran in the north, and Babylon, Nippur, Ur, and Borsippa in the south—collections that are adequately distributed in time to furnish essential controls. Further corroboration is offered by finds originating in scribal schools outside Mesopotamia proper, in which Akkadian and Sumerian were taught to foreign scribes in the course of their training.

With the exception of the late and highly technical astronomical texts from Babylonia proper, the contents of all these collections demonstrate that the picture offered by the library of Assurbanipal in Nineveh is basically representative. There exist, of course, the inevitable discrepancies and gaps. The laws of probability militate against the preservation of small text groups, and work havoc even with larger ones. In view of the fact that less than one-fourth of the body of traditional texts has been preserved, and then only too often in rather poor condition, and in view of the selection that is produced by the accidents of survival, of discovery, and—not to be underestimated—the accidents of publication, the picture of an over-all unity that results from the observation of these well-distributed collections entitles us to speak of the literary tablets of Mesopotamia as belonging to a coherent and continuous stream. When Assyriologists will be able to follow the fate of individual text groups through the history of their tradition, they will obtain more insight into the workings of this "stream" and, conceivably, light will be shed some day on ideological preferences and other attitudes that neither the content nor the wording of these texts is likely to reflect directly.

One more point bearing on the "stream of tradition" is to be discussed: what is the size of this body of texts?

The salient characteristic of all collections is the predominance of scholarly over literary texts, and, within the scholarly texts, the predominance of texts which the Assyriologists call "omen texts." Such omen collections consist of endless, systematically arranged one-line entries, each describing a specific act, a well-defined event, or the behavior or feature of an animal, a specific part of its body, of a plant, of a human being, also the movements of stars, the moon and the sun, atmospheric events, and other observable details of unbelievable variety. Each case is provided with a prediction that refers to the welfare of the country or to that of the individual with respect to whom, so is the basic assumption, the event happened, or for whom the observation was made if it was not purposefully provoked to obtain information about the future.

The library of Assurbanipal contained more than three hundred tablets, each holding 80 to 200 individ-

ual entries of the nature just described. Next in size seems to have been a group of about two hundred tablets of a quite different nature. These contain lists of cuneiform signs and sign combinations with added readings, and also lists of Sumerian words with their Akkadian translations, organized according to various principles of arrangement and representing to a large extent what may be termed a dictionary. They further include lists explaining rare and foreign expressions in Akkadian. In short, this group of tablets embraces in an encyclopaedic form everything required for teaching scribes the native (Akkadian) and the traditional (Sumerian) languages. The bilingualism of the scribes is reflected in a large number of Sumerian incantations and prayers that are provided with interlinear Akkadian translations. The latter form a group that seems to have amounted to more than one hundred tablets. About the same number of tablets contain cycles of conjurations for cathartic and apotropaic purposes, as well as what is customarily called the "epic literature," fables, proverbs, and sundry small collections of varia and trivia that somehow have found their way into the body of "canonical" texts. For reasons that will become clear presently, one should stress that the epic literature (such as the Creation story, the Epic of Gilgamesh, of Irra, the stories of Etana, Zû, etc.) amounts to only 35 to 40 among the seven hundred tablets so far enumerated.

The existence of about two hundred tablets more can be inferred with varying degrees of certainty from isolated fragments and other indications, such as catalogues of tablets, etc. As a safety margin dictated by a general pessimistic attitude rather than by rational considerations, one may add one-third again to these 930 tablets in order to achieve something like an informed guess at the total number of tablets kept in Assurbanipal's palace at Nineveh. One may perhaps—but not necessarily—assume that a further projection beyond this 1,200-tablet estimate should be hazarded, so that the figure 1,500 would represent, as a maximum, the entire body of cuneiform literature that included, at any time or place, part of what we are calling here the stream of tradition.

To venture further guesses, such as to the number of lines which these tablets may have contained, is sheer folly, but there is not too much doubt in my mind that the sum total would leave the Rigveda (about the size of the Iliad) and the Homeric epics, as well as the Old and New Testaments (which surpass the epics only slightly as to the number of verses), far behind, and would probably reach, if not exceed in bulk, even the size of the Mahabharata with its 190,000 verses.

It should be added that these figures refer to individual texts and not to the number of copies of these texts. In the royal library at Nineveh up to six exemplars are attested for the same text, which is often enough a great help in filling in lacunae and in reconstructing compositions. Since it was an essential part of the training of the apprentice scribes to copy tablets, those works that make up the primary curriculum are preserved in many more copies than those that are part of the higher levels of training which only a small number of students attained.

It now behooves us to outline what should be con-

sidered the characteristic features of this corpus of texts, surveying it without the professionally myopic outlook of the Assyriologist.

First one has to point out that nearly all of these 1,200 or more tablets were at some early point in their history frozen into a specific wording and an established arrangement of content. This process of standardization began quite early (third quarter of the second millennium B.C.) for certain key text groups—especially those of the encyclopaedic genre. It continued, successively affecting other groups, up to the time when the scribes of Assurbanipal assembled and copied individual tablets or small groups that had been in restricted circulation, and combined them into topical arrangements, giving them definite titles and indicating their sequence by numbers.

The standardization effectively maintained the original contents against the pressures of changing concepts and attitudes, preserving obsolete text material that would otherwise have certainly disappeared. For the Assyriologist this standardization is the greatest boon. Normally, all he has to work with are shattered fragments of tablets that come from several excavations and accidental finds, which more often than not contain lines that break off in the middle of the text, or which contain only beginnings and ends of lines. But due to the fact that nearly all identifiable fragments, wherever they come from, go back to one standardized version, the Assyriologist is often able to reconstruct an entire text out of small fragments.

The contents of all these tablets of the stream of tradition clearly indicate that the cuneiform literature which the Mesopotamians themselves considered essential and worthy of being handed down, concerned directly or indirectly the activities of the diviners and of the priests specializing in exorcistic techniques. Only a very small section contains what we, immersed in the Western tradition, like to call products of literary creativeness. One may, in fact, reasonably estimate to be fifty or sixty, at most, the number of tablets that contain what we are wont to call epic texts, including fables and rather platitudinous concoctions of practical "wisdom," as well as some tablets with prayers, etc., whose diction and imagery seem to us to be distinguished by a certain tang of genuineness, though it is open to some doubt whether this quality was instrumental in their inclusion into the stream of tradition.

The epic texts make a strong appeal to the esthetic tastes and ideological preferences of the Western cultures, steeped as these are in the literary and the religious traditions that originated in Greece and in the habitat of the Bible and were transposed into a new key in mediaeval Europe. This has induced us, consciously or not, to make two obvious mistakes: we have been exaggerating the importance of such texts, although they are only few and far between in the Mesopotamian literature, and—we are judging the main bulk of the tradition on the basis of the lack of texts which we are conditioned to appreciate.

There is a noticeable absence of historical literature, in the sense that texts are lacking that would attest to the awareness of the scribes of the existence of an historic continuum in the Mesopotamian civilization of which they themselves and their tradition were only a

part. To be sure, there are preserved a few late chronicles, a number of copies of very old royal inscriptions, a small group of texts that contain legends of early kings, and theological interpretations of sundry historic events of the pre-standardization period. Nothing, however, was considered worthy of recording that would relate the literary and intellectual traditions in and for which these scribes lived, with any co-ordinates of time, space, and socio-economic realities.

The same detachment expresses itself in the complete absence of any polemic in this type of literature. All statements appear without relation to any background of ideological, religious, or even political stress or tension. This is not for lack of opportunity, because the ritual complaints in the prayers written or adapted for royal use, or the predictions in the innumerable omen passages, could easily reflect discontent, social criticism, etc. Such tensions are very much in evidence in Greek texts, where they are further accentuated by the didactic style of scholarly presentations. There was apparently no rivalry between schools, nor clash between the Mesopotamian scribe's cultural outlook and that of those who lived around him, either in his own country or elsewhere. It is especially the latter contrast that imparts a very specific mood and intensity, in the Old and the New Testament, not only to pragmatic utterances but even to descriptive passages. The person of the scribe, his beliefs and ambitions, are conspicuously absent in cuneiform literature; no cognizance is taken of religious or philosophical insights; no constructive political thoughts are revealed, nor any awareness of man's role and pretensions in this world.

The explanation for all this is quite simple. What we have at hand in these 1,200 or more tablets is but a reference library geared to the needs of the diviners and those specialized practitioners of magic who were responsible for the spiritual security of kings and important persons. To this were added several sets of handbooks for educational and research purposes, meant to maintain the scholarly standards and the technical proficiency of these essential professions. By accident and hardly for what we would call their merits, literary texts were carried along in the stream of tradition as part and parcel of the education of the scribes simply because the copying of such texts belonged to the traditional curriculum.

The corpus has to be understood, appreciated, and utilized solely in terms of what it was meant to represent for those who created, maintained, and used it. And the literary texts have to be considered primarily from the point of view of their own position of importance within the stream of the tradition.

The Assyriologists, however, always did, and still do, approach them from a quite different angle. They look for deeply meaningful cosmologies, for *primaeva* wisdom, for the pomp of mythological exploits, the charm or crudeness of "early" social and economic patterns that supposedly reflect the growth of ideas beyond the ken of history, for legends and "historiae" and titillatingly different mores—in short, for what Western scholars in the "study of man" ever since Herodotus have expected to discover at the periphery of their

own, and of course normative, world. And expectations of that sort are apparently fulfilled to judge from the books produced by popularizers concerned with the Mesopotamian civilization. Such an attitude still today affects serious Assyriological research work in varying degrees. There are scholars who are inextricably entangled in attempts to relate Assyriological data to the Old Testament in some acceptable way, and others who find in haphazardly collected instances, torn out of their ideological and stylistic habitat, convincing proof for whatever the fashion of the day in anthropology, the history of religion, or economics is propounding. Even linguistically, the cuneiform texts have not been allowed to be subjected to candid and unbiased investigation. Having been, quite early and correctly, tagged as a Semitic language, the Akkadian was, and still is, mercilessly put on the procrustean bed of this or that other Semitic language that is whimsically considered normative.

Quite often this is done, not out of methodological considerations or on account of the objective range of the scholar's interest, but for reasons which seem rather to originate in a quest for a *raison d'être* for the entire field of Assyriology, not only in the eyes of other disciplines but also in those of the scholars themselves. This psychological situation has yielded, and still does yield, a number of specifically biased articles or even books, which can usually be recognized as such. The same situation, however, influences the research range of the Assyriologists in a more subtle way. It exerts considerable influence—normally at a subconscious level—on the selection of topics. Thus it creates or fosters preferences for certain literary patterns, mythological motifs, or social and economic contexts that in some way either correspond to, or are strikingly different from, those to which our composite Western background has conditioned these scholars.

Let us return to the literary texts of the stream of tradition. Any evaluation of them with respect to topic inventory and style types should bear in mind that there exists meagre, but unquestionable, evidence for a rich and productive oral literary tradition in Mesopotamia. It seems to have flourished not only before the period in which the standardization, or "canonization" of the written tradition became effective, but also parallel and subsequent to it. We know, for instance, of the existence of cycles of songs, mainly love songs, that were cast, in the fashion of the ancient Near East, in a specifically intense and quasi-religious phraseology, but also of songs sung in battle, in praise of the king, etc. We know further of courtly tales and legends spun around loved and feared kings, of popular stories with sometimes jocular and pungent undertones. There were in circulation dire prophecies and political diatribes in poetic form, riddles and animal tales, etc. Of all this we are informed mainly by isolated tablets, containing texts that do not belong to the stream of tradition and were written only accidentally and mostly in unique copies. Nevertheless, the very fact that these have survived entitles us to assume the existence of several literary genres that belonged to a tradition different in content and probably also in purpose from the written tradition discussed to this point. It is too simple to call that other tradition "oral," because the possibility has

to be considered that a divergence between the written and the "oral" tradition was the consequence of either technological (writing material) or linguistic conditions.

Let us first raise the question as to the social habitat of this type of literature, its carriers and its public. As a habitat outside the stratum in which the stream of the written tradition was in evidence, one could reasonably suggest the court of the kings of Babylon. The reason why we know next to nothing of the important and natural center of political, economic, and social life that must have existed there, is simple: no literary text of importance came to light during the excavations of Babylon (due to the rise of the water table in that region), and no archaeologist has ever happened to find the ruins of a Babylonian palace. We do know, however, that the courts of the kings of Ur, Isin, Larsa, and Babylon harbored both scholars and poets in the second millennium, and there is no reason to suppose that it was any different in the first, although there are hardly any indications available as to this role of the royal court of Babylon. There are several possible reasons to which this scarcity of documentation could be attributed: the lack of finds from Babylon, the use of perishable wax-covered tablets that may go back further in history than we are now assuming, and the possibility that the Aramaic language became, in Babylonia, at an earlier stage than generally supposed, the vehicle for a literary tradition different from that written in Akkadian and on clay tablets.

Such suggestions are offered here solely to illustrate the essential fact that the traditional cuneiform which we have been discussing should not be considered the main or only product of the creative effort of the Mesopotamian civilization. For its correct evaluation and an appreciation of its achievements and its importance, one has to realize its limitations in purpose, style, and content. One has to concede the existence of other types of literature in that civilization, genres that are of still undefined range, status, and import, even though the evidence is slim and circumstantial.

By no means do the traditional texts offer the most important documentary material for the work of the Assyriologist. There exist—and very often deservedly in the front ranks of interest—an impressive bulk of cuneiform tablets that contain the records of the day-to-day activities of the inhabitants of Mesopotamia, from kings down to shepherds. In time-span and geographical distribution, in bulk and in topical variety, they quite often surpass the traditional texts.

These tablets fall into two sharply differing categories: records and letters. The records deal overwhelmingly with administrative transactions of all sorts and originated in the realm of an elaborate bureaucracy that handled with technical skill and methodical consistency the affairs of the temple administrations of southern Babylonia (from Ur to Sippar, and from the end of the third to the last third of the first millennium B.C.). Such records were also used in the royal palaces all over the ancient Near East, wherever the Akkadian language and the cuneiform system of writing was in use, i.e., from Susa north of the Persian Gulf, to Alalakh near the Mediterranean coast. To a much lesser extent, these tablets record private legal transactions, such as

sales, rentals, loans, also marriages, adoptions, wills, etc. There exist further a number of international agreements on documents that are scattered through a period of one millennium.

The letters likewise fall into two groups, those dealing with administrative and political matters, and those that are concerned with private and personal affairs. The latter are far less numerous and restricted to specific periods and contexts.

We again feel obliged to venture a reasonable guess as to the number of these records and letters. It can be said that the material already published, together with that known to be kept from publication for a number of reasons by the several larger museums, amounts to about 30,000 to 40,000 tablets. This estimate refers to tablets written in, or predominantly in, Akkadian. Sumerian administrative and legal documents may run easily to more than three times that number.

What information do these texts contain? How and to what degree can this information be utilized for the understanding of Mesopotamian life and customs? Is this the raw material the historians of law and of economic institutions dream of? Is it these texts that will clearly reveal what those who wrote them and those for whom they were written, thought about themselves, their world, their gods?

Unfortunately, clear and easy answers to these questions cannot be expected. The potential usefulness of this source of information is severely curtailed by a number of factors. These texts cover a wide area geographically and a very long period of time, so that their large number is sharply reduced when one's research focuses upon a specific point in time and space and upon a specific problem. Again, the coverage of these texts is very irregular. Large areas and periods are blacked out for a variety of reasons, and only exceptionally is it possible to obtain insight into developments on a larger scale in time, or into regional differences on a synchronic level. The picture that any investigation based on such material can obtain consists of a number of mostly disconnected spots of light. It is as if a narrowly confined beam of light haphazardly illuminated this or that city between the Persian Gulf and the Mediterranean Sea at infrequent and irregular intervals during two millennia, leaving everything else in darkness. It is true that within the beam of that spotlight, complex institutions and political situations appear as the background in front of which we may observe history in the making—administrators at work collecting and appropriating taxes and services, merchants engaged in far-flung commercial activities, farmers and bankers arguing endlessly about debts, etc. Personalities appear quite often, the rise and fall of families can be observed, but, in most of the instances, for only two or three generations before darkness sets in again. Very rarely, where excavations have been persistent and fruitful or our luck has willed it, we have a sequence of such spotlights dotting the history of a city, such as in Nippur and Assur, also in Ur, and, to a certain extent, in Sippar.

An equally important obstacle to the utilization of this rich body of material is of a philological nature. This holds true, though for different reasons, for both the records and the letters.

Administrative documents were written solely for internal use; their diction is terse, abbreviated, and full of mysterious technical terms. It is a delicate and difficult task to establish the meanings of these terms that, in the course of time, quite often underwent subtle changes, and to reconstruct their institutional and economic background. Yet, only by doing so, could one hope to infuse some life into the strictly formalistic style of ledgers, lists, and receipts. Without a carefully established frame of reference, i.e., without our knowing who delivered and who received, and under what title and claim goods and services were allocated, administrative texts yield only a meagre harvest of personal names, a technical vocabulary elaborately describing staples and raw materials, and an opaque residue of unintelligible words from the bureaucratic lingo of the place and period.

Quite different, but equally forbidding, are the philological difficulties that hamper the study of the letters. Most of them are written by, to, and for officials, including the king. Their topics are reports, requests, and executive orders in administrative and legal matters; their diction ranges from voluble protests and insincere excuses to cutting remarks and invective. In the private letters—and there alone in cuneiform texts—we often come in contact with the spoken language, instead of the formalized phraseology of religious texts, the technical jargon of the scholarly literature, and the carefully archaizing and stylized verbiage of the historical texts. In quick-shifting, emotion-charged, but often quite pregnant sentences, topics are taken up and abruptly dropped, and allusions abound to situations known only to both correspondents. Emphasis, irony, rhetorical questions, veiled threats, unfinished sentences, and imprecations run a gamut of syntactic finesses to mold the diction of these letters to such expressiveness that it often remains beyond the ken of the philologist reared in the unreal and inane formalism of the conventional literary texts.

This characterization of the text material available in cuneiform sources has left one rather substantial group aside, the historical texts. This term is commonly applied to the royal inscriptions on which most of what we know of Mesopotamian history is based. They represent an important and valuable source material, but when one searches them for information other than names of kings and places, for more insight than can be offered by repetitious descriptions of victories and the pompous phraseology of triumph, one remains disappointed. The reason lies in two important stylistic features of these royal inscriptions which are always overlooked. First, only a rather small fraction of these documents was meant to be read for the purpose of recording and conveying information; instead, they were buried carefully in the foundations of temples and palaces, or placed inaccessibly in buildings or on rocks. Second, they are basically styled as communications of the king to his deity, reporting on warlike deeds and building activities performed. This is especially true for the younger group of Assyrian and Babylonian royal inscriptions which represents an ingenious adaptation of an earlier prototype that, fundamentally, took the

CROSS-CIVILIZATIONAL CHRONOLOGICAL CHART FOR THE ANCIENT NEAR EAST

3000 B.C. to 200 A.D.

AEGEAN AND GREECE	EGYPT	ASIA MINOR	SYRIA AND PALESTINE	ASIA
3000				
2900	<i>Egyptian writing</i>			
2800	Unification of Upper and Lower Egypt			
2700	Old Kingdom			
2600	Great pyramids			
2500				
2400				
2300				
2200	First Intermediary Period			
2100				
2000		City states	City states	Ba
1900	Middle Kingdom			
1800 <i>Minoan Linear A</i>		Assyrian traders		Co
1700 <i>Minoan Linear B</i>	Second Intermediary Period		Alalakh	San
1600	Hyksos domination	Old Hittite kingdom		
1500	New Kingdom			
1400		New Hittite Empire	Mitanni domination	Mi
1300	Akhnaton	Šuppiluliuma <i>Hieroglyphic Hittite writing</i>	<i>Alphabet—Amarna Period</i>	Tu
1200	Ramses II			Ti
1100		Phrygian invasion		
1000 <i>Greek alphabet</i>	End of the Ramessides			
900	Libyan domination		David in Jerusalem	As
800 Homer				San
700	Ethiopian domination		Fall of Samaria	As
600	Assyrian invasion	Gyges		v
500	Persian domination	Croesus	Fall of Jerusalem	Per
400 Age of Pericles				Gr
300 Alexander	Greek domination—Ptolemies	Greek domination	Greek domination	S
200				
100				
B.C. I			<i>Jesus Christ</i>	
A.D.				Par
				Sas

ASSYRIA	BABYLONIA	IRAN	INDIA
	<i>Cuneiform writing</i>		3000
	Rise of Nippur	<i>Proto-Elamite writing</i>	2900 2800
	Early city states		2700 2600 2500
	Sargon of Agade		<i>Indus Valley writing</i> 2400 and civilization 2300 2200 2100 2000
Babylonian governors in Assur	Empire of Ur III	Babylonian governors in Susa	
Commercial expansion Šamši-Adad I	Later city states Rise of Babylon Hammurapi Kassite invasion and rulers	Old Elamite kingdom	1900 1800 1700 1600
Mitanni domination	Period of international contacts		Vedic period 1500 1400 1300
Tukulti-Ninurta I	Conflicts with Assyria	Šutruk-Nahhunte I (of Elam)	1200
Tiglath-Pileser I	Nebuchadnezzar I Aramaic invasion		1100 1000
Aššurnasirpal II Sargon II Assurbanipal (Fall of Nine- veh)	Assyrian domination Rise of the Chaldean dynasty— Nebuchadnezzar II Nabonidus—Fall of Babylon— Cyrus II Persian domination Greek domination—Seleucids	<i>Zoroaster</i> Cyrus I Darius I Greek domination	900 800 700 600 500 400 300 Aśoka 200 100 I B.C.
Parthian domination Sassanian domination	Parthian domination Sassanian domination	Parthian domination Sassanian domination	A.D.

form of a votive inscription. As such, these historical inscriptions are extremely interesting, but with regard to available information their yield is rather poor. In combination with king lists and treaties they may well serve to roughly outline the course of historic events, but they cannot bring us any nearer to an understanding of Mesopotamian history. From what social, economic, or other situations sprang the aggressive *élan* of Assyria, the tenacity and the staying power of Babylonia? What pressures guided the continuous struggle of both civilizations in their search for a livable and workable form in which their political and spiritual preferences could materialize with that stability which was to them an eternal dream and which eluded both of them time and again?

Documentary evidence of the type here described can be handled in two ways: either through a process of sustained synthesizing on a specific and restricted level or approach that singles out certain data, and analyzes and interprets them in detail, or, also, through an overall synthesis that aims at the creation and constant re-creation of a picture that is to embrace the entire civilization, either diachronically or synchronically. The latter kind of synthesis should give direction and impetus to further research by pointing out the frontiers of knowledge and convey, ultimately, an image of the field, of work done, in progress, or to be desired, to both the Assyriologist and all scholars who care to know about Assyriology.

In both these kinds of synthesis, we have had little effort and less success. With regard to the first kind, one has to remember that the Assyriologist has at his disposal but a small section of material. Any new excavation and any other find can endanger and overthrow the conclusions he has reached. This can place a severe strain on the creative activity and the scholarly *élan* of those who shy away from the traumatic experience of being compelled to discard carefully worked out conclusions. Of course, the classical scholar may also have to face new and surprising data, but they cannot be compared in scope and relevance with what the Assyriologist has every right to expect. Another hazard, touched on above, concerns the difficulty of synthesizing data coming from a deeply alien civilization, a civilization that is reflected solely in the dull and distorting mirror of documents written in a dead language. It is necessary, but extremely difficult, to free oneself consciously and consistently from one's own ingrained conceptual conditioning in order to organize adequately any data pertaining to an alien civilization. How else can a Western scholar evaluate the tenor, mood, and sincerity of a polytheistic religion, or comprehend the delicate complexities in the workings of alien institutions which articulate the social and economic practices for which he has at hand nothing but some shattered documents that only accidentally can shed light on the numerous questions he has to ask? And if the wrong questions are posed, whatever answer can be obtained will be wrong or, at least, misleading.

With regard to an over-all synthesis that purports to embrace the entire field, the following procedure has usually been applied. All extant data that can be easily and, mostly, uncritically collected are projected, in complete disregard for chronological, regional, and con-

textual differences, upon one level in time and one dimension in space within the framework of a grid that reflects nothing but the cultural background of the scholar at work. When one thus "synchronizes" and "consolidates" a not too varied array of data, one can rather easily achieve what the undemanding and the outsider would term reasonable coverage. When all data are summarily pigeonholed into the conventional framework of such headings as "king," "temple," "religious life," "mythology," "magic," "family," etc., the goal of the presentation is considered reached. It is, of course, easy to shrug one's shoulders over such glib popularizations and leave them to marginal scholars and loquacious archaeologists, but one has to confess that this is an attitude of the Assyriologist that borders in many respects on cowardice. The battle for synthesis is the battle to be fought by him, and this battle should be considered his *raison d'être*, even though it is a battle that can know no victorious outcome. The battle as such must be the task of the Assyriologist.

Typically, however, we prefer to escape into peripheral skirmishes. The field of Assyriology has grown so wide and complex that not more than a handful of scholars can claim to be at home in its manifold domains. Most of the Assyriologists restrict their interest to apparently well-documented subdivisions and often select in premature specialization a specific area as their field of research. All this is to a certain extent due to psychological reasons: such work is more likely to yield a feeling of satisfaction, achievement, and security than the continuous endeavor to keep abreast of the incessant changes created by the afflux of new texts, new interpretations, and new meanings. Consequently the scholarly journals in the field of Assyriology contain mainly learned editions of individual texts, if not fragments of texts and of small groups of documents, and technical discussions of minutiae and of a selection of small-scale problems that happen to be the fashion of the day. Even important additions to our text material are rarely presented in systematic correlation to an over-all frame of reference.

If what has been said up to now sounds to the reader like a longwinded preamble intended to offer the Assyriologist a panacea, a new deal, a new way, let him be assured that I do not believe that the diagnosis of our malaise allows any such simple medication.

There are, however, indications as to the direction in which one may have to look to remedy the situation here outlined. The spectacular successes in the interpretation of cuneiform texts dealing with mathematics and astronomy are quite obviously the result of close co-operation between the Assyriologist and the mathematician and astronomer interested in the history of his discipline. And it is no accident that in both these instances the initiative came from the outside. Similar if not so spectacular successes have been experienced in the study of the legal documents from Mesopotamia, in which case the stimulus came likewise from the historian of law.

This may, at last, be the solution of many problems that beset Assyriology. Perhaps the descriptive linguist will help us throw off the fetters that are hampering our progress in the understanding of both the Sumerian and the Akkadian languages, the historian of medicine

may well contribute essentially towards the digestion of the numerous medical texts in cuneiform at our disposal that so far have never received any adequate treatment, and the historian of technology will show us the way in which we should investigate, e.g., the tablets describing the manufacture of colored glasses and to understand the elaborate technical terminology referring to metallurgy, etc. But one must not stop at the physical sciences in this respect. Assyriology direly needs the understanding and sustained co-operation of interested scholars in economics, the social sciences, and, above all, in cultural anthropology, in order to penetrate towards a better understanding of the institutional structure of Mesopotamia and especially of the religion, or better, the religions, of the entire region that have left us their reflection in uncounted documents.

And the Assyriologist need not be afraid that his discipline will enjoy only an ancillary role in such collaborations—quite the opposite will be the consequence. No history of technology that claims scholarly status can be written when its author has to rely on inadequate and mostly incorrect translations of cuneiform texts pertaining to his subject or on the worthless rehash of popularizers, and that holds true for all the mentioned disciplines. The Assyriologist should become aware that he holds the keys to a potential wealth of information covering far more than two millennia of one of the first great civilizations. If he is in need of a *raison d'être*—here it is.

All this is not meant to be a "programme," but neither should it be simply called wishful thinking—it is a way, well worth considering, out of the stagnation from which we suffer, a stagnation of which the most salient symptoms are the shrinkage of topics selected for research, the "flight into specialization," and the scarcity of students who once used to stray over quite frequently from theology into the greener pasture of a new and venturesome discipline.

If the new directions here surveyed mean that Assyriology will eventually move from the Humanities into cultural anthropology, I shall shed no tear. The Humanities have never been quite successful in treating alien civilizations with that tender care and deep respect that such undertaking demands. Their conceptual tools have been and still are geared for integration on their own terms and for assimilation along Western standards.

Let I be accused of daydreaming here, or, worse, of preaching, let me point out, in a necessarily very subjective way, where I see the real frontiers of Assyriology. Since Assyriology is a field that is neither favored by the interest of even a sophisticated public nor within the range of the intellectual preferences of the day, very few scholars are in a position to express themselves freely in books because books are expensive to print and scholars have therefore to follow certain patterns and topical considerations in order to obtain the necessary financial assistance. If one seriously wants to obtain information as to the predominant interests of the scholars in the field, their aspirations, standards, and methodological orientation, one has to turn to the articles, book-reviews, etc., that are published in a constant stream in a number of scholarly

periodicals of the United States, Europe, and Asia. In their variety they reflect quite truly the ever-shifting direction of topical predilections, the interplay of schools of thought, of methods, etc.

When one leafs through these periodicals one can hardly fail to discover in that medley of learned miscellanea, texts translated and studied, problems discussed, etc., certain areas of avoidance which the presence of preferred preoccupations makes the more obvious to the interested observer. Here is a random selection of tabooed topics.

Philological problems are hardly touched upon—and if they are, only in the style evolved in the nineteenth century. No revolutionary is in sight to shatter the happy quiet of complacent stagnation and to turn to factual data and truly descriptive categories to blast away the superimposed constructions of the conventional grammars. The avoidance of historical studies, beyond the level of the necessary constant adjustment of the chronological framework, is made somewhat less obvious by the smoke screen of the clichés on which the historians of the last century thrived, such as migrations, dark ages, intermediate periods, etc. Little if any thought is given to the essentially literary problem of the evaluation of the documentary evidence from Mesopotamia and its relation to the historical events. The well-documented complexities of the social and economic life of the region are moreover levelled by assuming unilinear developments or equally unfounded alternations of periods of flowering and catastrophies, and the intricate polyphony of the political life of Assyria and Babylonia are sadly missed by the would-be historians who listen solely to the *bassi ostinati* of the royal reports on battles, booty, and victories. The fascinating problem of the rise and development of Mesopotamian civilization out of a very early fusion of several streams—of which we are wont to identify only two, the Sumerian and the Akkadian—is likewise generally shunned. The Sumerologists claim whatever is recorded in that language simply as representative of an indigenous Sumerian civilization. Any serious attempt to approach these questions cannot respect language barriers, however, and that should likewise hold true with respect to the Akkadian-speaking Semites and to other layers of Semites whose specific contributions to the social and political structure of Mesopotamia have not yet been established. Above all, one has to take into consideration that there were unidentified languages in the region that betray their existence and importance by an array of geographical, divine, and personal names, as well as an impressive number of technical terms referring to religious and social institutions, essential tools, plants, animals, and technological achievements.

A pall of silence has been allowed to settle in the last decennia over that field of research that had so much appeal in the olden days of Assyriology, the religion of Mesopotamia. The days are, happily, over when the reader of books on this topic was mercilessly exposed to lengthy enumerations of the gods of the teeming pantheon, to which were added inevitably the contents of appropriate mythological stories—no religion

was considered acceptably presented without the poetry and the charm of myths. Then came the designations and supposed functions of what were generously termed priests, and descriptions of festivals and ceremonies of sundry proveniences and periods. Occasionally, there was a chapter on morals, which meant, as a rule, a listing of those data of which the writer's personal code of morals disapproved. Equally pernicious—but much shorter—are “bird's-eye-view” type presentations that use preconceived patterns to produce a picture that highlights certain selected aspects allowing the haze of distance to engulf the rest. Above all, there is a distinct lack of respect among the scholars studying Mesopotamian religion for the unbelievable complexity of this civilization.

There is no “religion” of Mesopotamia to be studied, but a coherent and yet distinct group of manifestations each in its social, regional, and cultic set-up. The religion of the theologians, that of the king and his court, of the city dweller, of the farmer on one hand, those of the south and the north, of the peripheral regions, the plains, the piedmont tracts on the other, those of the small decaying cities, the prosperous capitals, the powerful temples, etc., they all have their internal development, their contacts and antagonisms, which have to be studied, individually and carefully, to yield the information available.

Practically untouched remains, finally, the problem of interrelating Mesopotamian civilization to the world

around it, whether it was giving or taking. These relations materialize on many levels, such as domesticated plants and animals, heat technology (from copper to frit), tools, weapons, crafts, architecture, and communication techniques, and are in evidence in varying degrees of intensity and in opposing directions from the fourth millennium B.C. into the beginning of the first A.D. Archaeology and philology have to combine forces to trace such connections, and the field of work will have to extend from Lybia to the Pamir and from the Caspian Sea to the sources of the Nile—and even these borderlines will have to be crossed at times. Far more difficult will it be to relate institutions—those which co-ordinate the individual into the group, the subjects to their ruler, men to their gods, or the patterns of expression that formalize man's spiritual expectations and apprehensions, or to trace the borderlines that indicate what, or who, is sacred, what, or who, has power. Like the visible parts of an iceberg, only one-ninth of these data are ever reflected in writing; the balance remains hidden forever in the night of the previous periods.

This list can be lengthened at will. What it is meant to convey is solely the conclusion that Assyriology is still that rich and promising field of research that it ever was, although its frontiers are not as obvious as they used to be. From conquest and incorporation, the Assyriologist will have to turn, sooner or later, to integration and penetration.

A Bibliographical Note

It is the purpose of this note to offer the interested reader the possibility of informing himself, on a not too technical level, about work going on in the field of Assyriology. Care has been taken to mention wherever possible books that are generally available, even if they fail to meet certain scholarly standards. Books that contain extensive surveys on work done in specific sub-fields of Assyriology have been given preference. Articles published in scholarly periodicals are only exceptionally quoted; the reader will have to search the bibliographical surveys mentioned below to obtain additional information.

For the person who wants to obtain a well-balanced and straightforward, if somewhat pedantic, view of Mesopotamian civilization in most of its aspects, B. MEISSNER's *Babylonien und Assyrien* (Heidelberg: C. Winter, 1920 and 1925; 2 vols.) still offers more reliable information than all the more recent books of this kind, which unfailingly use secondary if not tertiary sources. No comprehensive book on the peoples of the ancient Near East is available. Strange as it may seem to the outsider, the several Semitic-speaking peoples that have populated Mesopotamia from the third millennium on—the Akkadians and the several subse-

quent waves of immigrants and invaders, including the Arameans and the Chaldeans—have not been made the subject of an extensive study by any competent scholar from the point of view of cultural or physical anthropology. S. MOSCATI's *Ancient Semitic Civilizations* (London: Elek Books, 1957), however, offers a resumé of the assumptions that have currency at the moment. For the Sumerians we have an enthusiastic presentation in S. N. KRAMER's *History Begins at Sumer* (London: Thames and Hudson, 1958), and one may refer also to H. SCHMÖKEL's *Das Land Sumer* (Stuttgart: Kohlhammer, 1956; 2nd ed.), though it is removed from any direct contact with original text material. For the Hurrians, the instructive and readable book of A. GOETZE, *Hethiter, Churriter und Assyrer* (Oslo: H. Aschehoug and Co., 1935), and the more technical presentation of I. J. GELB, *Hurrians and Subarians* (Chicago: University of Chicago Press, 1944), have to be mentioned.

The story of the decipherment of the cuneiform systems of writing and that of the excavations in and around Mesopotamia are well and extensively presented in a recent book of A. PALLIS, *The Antiquity of Iraq* (Copenhagen: E. Munksgaard, 1956), Chapters II and III. One may compare A. PARROT's *Archéologie Mésopotamienne* (Paris: A. Michel, 1946), written more from the

point of view of the archaeologist, that is, the French archaeologist.

Two books which will familiarize the reader with the history of the region on a more or less elementary level should be suggested here. There is, first, L. DELAPORTE's *Le proche-orient asiatique* (Paris: Les Presses Universitaires de France, 1938), and, with less reference to original text material and pertinent research work, A. MOORTGAT's “Geschichte Vorderasiens bis zum Hellenismus,” in A. Scharff and A. Moortgat, *Ägypten und Vorderasien im Altertum* (München: F. Bruckmann, 1950). A recently published, though rather specialized, study of D. O. EDZARD, *Die “zweite Zwischenzeit” Babylonien* (Wiesbaden: O. Harrassowitz, 1957), should be mentioned as a harbinger of a renewed interest in history on the part of those who are able to read the texts bearing on it. The present status of the much discussed problems of the chronology of the second millennium B.C. is summed up in M. B. ROWTON, “The Date of Hammurabi” (*Journal of Near Eastern Studies* [1958] 17: 97-111.)

There does not—and, most likely, should not—exist any book with the grandiose aim of presenting in an up-to-date way what is customarily referred to as “Mesopotamian,” or “Assyro-Babylonian,” religion. The book of E. DHORME, *Les religions de Babylonie et d'Assyrie* (Paris: Les Presses Univer-

sitaires de France, 1945), belongs to the old-fashioned approach to the complex of problems and resorts to appalling simplifications in order to be able to deal with them. As a characteristic example of an attempt to use a much wider angle and a more abstract outlook, we may quote here T. JACOBSEN's "Mesopotamia," in H. and H. A. FRANKFORT, J. A. WILSON, T. JACOBSEN, and W. A. IRWIN, *The Intellectual Adventure of Ancient Man* (Chicago: University of Chicago Press, 1946), also to be found in *Before Philosophy* (Penguin Books A 198). As a sample of a very personal approach, we may quote C. J. GADD's *Ideas of Divine Rule in the Ancient East* (London: Oxford University Press [for the British Academy], 1948).

Among the very few books that deal primarily with the source material bearing on Mesopotamian thought and world view but, at the same time, take cognizance of the intellectual and literary problems involved are W. G. LAMBERT's *Babylonian Wisdom Literature* (Cambridge: Cambridge University Press, 1960), and A. L. OPPENHEIM's *The Interpretation of Dreams in the Ancient Near East* (Transactions of the American Philosophical Society 46, Pt. 3 [Philadelphia, 1956]).

The artistic creativeness of Mesopotamian civilization manifests itself most effectively in the statuary, the reliefs, and the glyptic; it has found a masterly evaluation and impressive presentation in H. FRANKFORT's *The Art and the Architecture of the Ancient Orient* (Pelican History of Art Z 7). Much less satisfactory is the situation with regard to Mesopotamian literature. For Sumerian, one may use the book of S. N. KRAMER cited above, but for Akkadian, in respect to which we have reached a higher degree of sophistication due partly to the fact that more, and more varied, texts are available, one can point out only a few and rather accidental discussions of literary texts of a specific genre such as, e. g., the introduction to A. FALKENSTEIN and W. VON SODEN, *Sumerische und akkadische Hymnen und Gebete* (Zürich: Artemis-Verlag, 1953). Apart from Meissner's survey (mentioned above), no thorough presentation of the topical range of cuneiform literature is available and, much less, any attempt at stylistic investigation, literary appreciation, or even a serious comparison with the literatures of other Semitic-speaking peoples or of other civilizations of the ancient Near East.

When one turns to the languages spoken in and around Mesopotamia, the fact that Assyriologists are by predilection and training philologists, becomes quite evident. There are many more pertinent books to mention here, and

the number of articles dealing with this topic is legion. A. FALKENSTEIN's *Das Sumerische* (Handbuch der Orientalistik; Leiden: E. J. Brill, 1959), and his more representative *Grammatik der Sprache Gudeas von Lagaš* (Rome: Pontificum Institutum Biblicum, 1949+; 2 vols.) offer the latest in the study of the Sumerian language. W. VON SODEN's *Grundriss der akkadischen Grammatik* (Rome: Pontificum Institutum Biblicum, 1952) will remain for a long time the basic tool of the Assyriologist. Even so, a purely descriptive presentation of Akkadian that would open up the language to the linguist is still lacking, and so is a grammar to be used by the beginner. Akkadian dictionaries have been few and far between for more than half a century; the usefulness of those published has been quickly reduced by the steady influx of new text material, even where they were not deficient from other points of view. The situation promises to be at long last remedied by the appearance of the extensive *Assyrian Dictionary* (ed. I. J. GELB et alii [Chicago: Oriental Institute, and Glückstadt: J. J. Augustin, 1956+], six of a projected 21 volumes having been already published or being now in press), and by the much shorter *Akkadisches Handwörterbuch* of W. VON SODEN, (Wiesbaden: O. Harrassowitz, 1959+; two fascicles published), which utilizes the collections made by the late B. Meissner. However, the Assyriologist will have to rely for many years to come on his own collections until the large projects are finished, and possibly after, unless provisions are made to keep the contents of these dictionaries abreast of new text material and the continuous progress in the field. A Sumerian dictionary in the proper sense of that term does not exist. The work of A. DEIMEL, *Sumerisches Lexikon* (Rome: Pontificum Institutum Biblicum, 1925+), although quite serviceable even today, is to be considered somewhat of a still useful relic of the early days of Assyriology.

Looking for a moment at the languages written in one or another of the cuneiform systems of writing in the ancient Near East, we may simply list: J. FRIEDRICH, *Einführung ins Urartäische* (Leipzig: J. C. Hinrichs, 1933); J. FRIEDRICH, *Hethitisches Elementarlexikon* (Heidelberg: C. Winter, 1940); C. H. GORDON, *Ugaritic Handbook* (Rome: Pontificum Institutum Biblicum, 1955); R. G. KENT, *Old Persian Grammar, Texts, Lexicon* (New Haven: American Oriental Society, 1950); ERICA REINER, *Elamite* (Handbuch der Orientalistik; Leiden: E. J. Brill, in press); and

E. A. SPEISER, *Introduction to Hurrian* (New Haven: American Schools of Oriental Research, 1941).

The several law codes, both in Sumerian and in Akkadian, as well as the numerous legal documents and other information concerning legal practices that are available in cuneiform texts, form an important domain in which Assyriology and the history of law and social institutions meet. The book of M. SAN NICOLÒ, *Beiträge zur Rechtsgeschichte im Bereiche der keilschriftlichen Rechtsquellen* (Oslo: H. Aschehoug, 1931), as well as two articles—P. KOSCHAKER, "Keilschriftrecht" (*Zeitschrift der Deutschen Morgenländischen Gesellschaft* [1935] 89:1–39) and the rather inaccessible G. CARDASCIA, "Splendeur et misère de l'assyriologie juridique" (*Annales Universitatis Saraviensis* [1954] 3:156–62)—offer much more than an introduction to that material and afford an up-to-date survey. Of course, social institutions, with the exception of kingship which is treated in some rather imaginative books, remain practically untouched.

For the important fields of the history of science, Assyriology contributes profuse data of unique antiquity. This is especially the case for the histories of mathematics, astronomy, and medicine. It so happens that the first two are covered with unparalleled excellence by O. NEUGEBAUER, *The Exact Sciences in Antiquity* (Providence: Brown University, 1957; 2nd ed.). The historian of medicine who looks for a serious introduction to cuneiform medical texts is much worse off. There are a small number of pertinent articles in periodicals, but there is no book that can claim to deal with that interesting topic with any degree of competence and reliability. In spite of some small-scale studies, this holds true also for technology, a field still waiting to be investigated by teams consisting of philologists and historians of technology.

A pioneering effort to link philological research to the sciences was made by B. LANDSBERGER through his collaboration with a zoologist in *Die Fauna des alten Mesopotamien* (Leipzig: S. Hirzel, 1934).

Some lines in this greatly compressed survey should finally be given to the civilizations that flourished in contact with, or under the influence of, Mesopotamia. For Asia Minor, which includes the Hittite and other civilizations of that region, we have a model handbook in A. GOETZE's *Kleinasien* (München: C. H. Beck, 1957); cf. also O. R. GURNEY, *The Hittites* (Pelican Books A 259). No informative and

scholarly guidebook is available for those interested in the civilizations of Urartu, in the mountains of Armenia, nor of Elam. The several ephemeral civilizations that grew at one time or another between the western bend of the Euphrates and the Mediterranean coast, and often used the Akkadian language and system of writing, remain likewise without convenient presentation. One should quote W. F. ALBRIGHT's *From Stone Age to Christianity* (Baltimore: Johns Hopkins, 1946; 2nd ed.) as the most readable introduction into the thorny problem of the relationship of the Bible to Mesopotamian civilization, and—for the easternmost contact zone—STUART PIGGOTT's *Pre-historic India* (Pelican Books A 205).

As to the periodicals mentioned above in the text (see pg. 415), some are wholly dedicated to Assyriology and related subjects, and the rest offer articles of Assyriological interest among others in the field of oriental studies. To the former belong, as the two oldest, the German *Zeitschrift für Assyriologie und verwandte Gebiete* (published since 1886) and the French *Revue d'Assyriologie et d'archéologie orientale* (likewise since 1886). Also in this group are the *Archiv für Orientforschung* (since 1923) and the *Journal of Cuneiform Studies* (since 1947). Others, such as the *Journal of Near Eastern Studies* (since 1942), *Iraq* (since 1934), *Sumer* (published in Baghdad since 1945), and *Anatolian Studies* (since 1951), present much Assyriological material, while quite a number of journals of orientalist societies, etc., offer pertinent articles from time to time. There exist two bibliographies that carefully keep track of these numerous articles: that contained in the *Archiv für Orientforschung*, which covers the period from 1925 until now and is organized in geographical and topical subdivisions, and that in the periodical *Orientalia N. S. [Nova Series]* published by the Pontificium Institutum Biblicum in Rome, which was begun in 1939 and contains periodic indexes of the names of authors and of topics.

The overwhelming majority of cuneiform texts are published by the large museums, such as the British Museum in London, the Musée du Louvre, Paris, the Staatliche Museen in Berlin, and the University Museum of the University of Pennsylvania, to mention only the largest collections, in extensive series of volumes that contain only handmade copies. This holds true also for most of the publications of smaller museums. Uncounted and often quite important texts have been scattered through the scholarly journals sometimes in quite unexpected places, and this makes life difficult for an Assyriolo-

gist who does not have at hand one of the very few first-class libraries. Sooner or later it will become imperative and indispensable to collect these texts in some form so as to make them easily available.

The natural question for any outsider to ask concerns the accessibility of all this material in translation. There are, of course, quite a number of books and articles in which certain texts, text groups, and even entire blocks of texts of the same and related nature have been translated. To list them systematically would, however, require the space of a book—which, by the way, is again one of the many desiderata in our field. More important even would be a systematic collection consisting of volumes offering historical, epical, ritual texts, the works of the diviners, prayers, etc., all presented in transliteration, translation, and with a minimal commentary for the use of scholars in other disciplines as well as of the Assyriologist. If carefully kept up, such a "Loeb Classical Library" for cuneiform texts would contribute far more to the advance of the field than many a costly expedition. Two such attempts were already made some fifty years ago ("Keilinschriftliche Bibliothek" and "Vorderasiatische Bibliothek"), but World War I and the constant influx of new texts saw the end of this effort.

As an anthology of translated and commented texts that would offer a representative cross-section of cuneiform literature in its manifold aspects, one can point out only the book edited by J. B. PRITCHARD, *Ancient Near Eastern Texts relating to the Old Testament*, (Princeton: Princeton University Press, 1955; 2nd ed.), of which some 190 pages are given to Assyriology. The value of the translated texts as an anthology is, however, greatly diminished by the fact that the material was assembled solely to illustrate relations to the Bible: the Egyptian and Hittite material that does not seem to have been subject to such a restriction offers therefore a far more representative selection.

Comments

By JAMES B. PRITCHARD*

This inventory of the kinds of material with which the Assyriologist works is particularly welcome since the stock-taking is done by one who views the wide range of cuneiform texts from the vantage point of the Assyrian Dictionary Project. The bibliographical note, with the author's well-tempered comments, is a valuable feature. I should like to second most enthusiastically his

proposal for the publication of a classical library of cuneiform texts. The project is entirely feasible, provided financial support can be found and younger Assyriologists can be persuaded to devote some of their energies to the task. Oppenheim's plea to scholars in the fields of economics, social science, and cultural anthropology for their co-operation in interpreting the written materials from Mesopotamia deserves to be applauded by the more traditional associates from the fields of history, archaeology, and comparative religions.

By ROBERT M. ADAMS*

Oppenheim's lucid and detached analysis of the subject matter of his discipline deals primarily with the obstacles in his cuneiform sources to interpretation and synthesis—the immense accidental gaps, the narrowness and artificiality of the recorded stream of tradition, the terseness of economic records and transactions. These reinforce tendencies, we are told, either to popularize without regard for regional and chronological differences, or to escape into specialization on peripheral issues. Citing the results of—also rather specialized—efforts at collaboration with Assyriology that have been initiated by other disciplines, he calls for sustained co-operation with cultural anthropologists "in order to penetrate towards a better understanding of the institutional structure of Mesopotamia and especially of the religion" Yet the character of this proposed co-operation on this broadest of all possible themes nowhere is made clear, beyond an enumeration of the complexities that will obstruct any renewed search for historical regularities or social, economic, and religious synthesis. Sharing Oppenheim's general sentiments, I might attempt further to define the role of anthropology and to contrast it with that of Assyriology in the increasing collaboration that is certain to come.

If the ultimate objective, synthesis, is recognized as common, the basic distinction becomes one of approach rather than outlook. Following Robert Redfield ("The Social Organization of Tradition," *Far Eastern Quarterly* 15:17), we might characterize Assyriological studies as primarily *textual*, and anthropological studies as primarily *contextual*. This is not meant to imply, however, that only anthropologists seek to generalize; after all, something like a "flight into specialization" is not unknown in anthropology. But it does mean that the major immediate concern of the philologist or humanist-historian is with penetrating the full depth of meaning in his textual sources, while that of the anthropologist is with seeking to estab-

lish regular patterns and relationships that reduce and most economically "explain" the complexity of his data.

Since few anthropologists have become familiar with even the secondary philological literature, it is easiest to illustrate the anthropologist's role with archeological material. He may turn to the results of excavations, for example, for the importance of militarism and the extent of civic planning as seen in the layout of ancient towns, for the differentiation between ceremonial, palace, and domestic precincts within them, for indications in grave-goods of the extent of social stratification, or for evidence on the approximate place of private commerce or on the specialization of the crafts. Again, archeological surveys have helped to place these towns in a wider pattern—changing over broad time-periods—of settlement based on ir-

rigation agriculture in a semi-arid environment.

The common feature of these examples is not that they are archeological in character; indeed, no anthropological syntheses would get very far without the simultaneous employment of the cuneiform sources. But the examples are essentially anthropological in that they depend primarily on a continuous check for "fit" between the observed phenomena, on the one hand, and their assumed cultural context, on the other, revising the interpretation of both as necessary. Moreover, all of them begin with aspects of the general background with which must somehow be integrated the less measured and comprehensive but far more vividly-illuminated find-

ings of the humanists. Although unfortunately it has not yet been seriously attempted, the same approach could fruitfully be applied to the study of changing forms of kinship and social organization, in this case from textual rather than from archeological sources. It is an approach, I suppose, which in some ultimate sense is classificatory and comparative rather than analytical, whether the units of comparison be categories like nuclear families, charismatic leadership, temple hierarchies, or entire civilizations.

Both textual and contextual approaches converge on synthesis, crossing different terrain but remaining interdependent and complementary. Therein lies the necessity for co-operation.

SOCIAL DIALECT AND LANGUAGE HISTORY

by William Bright

WITHIN ANY recognizable speech community, variations are normally found on all levels of linguistic structure—phonological, grammatical, and lexical. Some of these variations are correlated with geographical location: there are systematic differences, for instance, between the English of London and the English of New York. This type of linguistic variation has been studied in detail by dialectologists. Other types of linguistic variation, however, have received less attention. Some of this variation may be said to depend on the identity of the person spoken to or spoken about; the classical instances are those in Nootka, where separate linguistic forms are used in speaking to or about children, fat people, dwarfed people, hunchbacks, etc. (Sapir 1915). Other variations are correlated with the identity of the speaker. These include cases of difference between men's and women's speech, e.g. in Koasati (Haas 1944). More typically, linguistic variation is correlated with the social status of the speaker; this may be termed a variety of sociolinguistic variation. An instance which has recently received considerable attention is that involving "U" (upper-class) and "non-U" (middle-class) speech in England; it is claimed that the difference in speech has now become virtually the only overt mark of difference between these two classes (Ross 1954:20-23). This type of variation thus provides a potential diagnostic index to social status, though sociologists have exploited this potential very little so far.

It should be noted that some cases of linguistic variation are correlated simultaneously with the identity of the person spoken to and the identity of the person speaking. Thus "female speech" in Yana was used not only by women, but also by men in speaking to women; "male speech" was used only by men speaking to men (Sapir 1929). In the sociolinguistic area, linguistic variation often reflects the relation between the status of the speaker and the status of the person addressed, rather than the absolute status of either; an example is Vietnamese (Emeneau 1950:206-09).

Still other cases of linguistic variation

are correlated not primarily with the identity of persons, but with other factors in the social and cultural context. For instance, a special type of speech was used by the Chiricahua Apache when on the war-path (Opler and Hoijer, 1940). A type of variation which is familiar in most societies is correlated with the difference between formal and informal situations—"formality" and "informality" being defined, of course, in terms of each particular society. Thus, as most Americans can confirm, pronunciations like *huntin'* are found more commonly in informal situations, while pronunciations like *hunting* are more common in formal situations (Fischer 1958:50). Variations such as this one in English are, to be sure, usually correlated with other factors besides that of formality. In some languages, however, the styles of speech used in formal vs. informal situations are highly standardized and strictly differentiated. Ferguson (1959) has applied the term *diglossia* to this type of linguistic variation, and has described it in the Arabic, Swiss German, Haitian French, and modern Greek language communities. We may consider this another type of sociolinguistic variation, correlated with the varying social context within which an individual communicates. Here too, the linguistic differences provide a potential means for definition and recognition of social situations.

The Indian subcontinent is an exceptionally good field for the study of both types of sociolinguistic variation, and a volume describing such phenomena in several South Asian languages is now being issued (Ferguson and Gumperz 1960). First of all, the Indian caste system makes for easy recognition of the social levels with which linguistic variation is correlated. Thus, in the Dharwar District of Mysore State, "there appear to be three styles of conversational Kannada which correspond to the three main cleavages in the social system . . . the Brahmin, the non-Brahmin, and the Harijan ['untouchable']" (McCormack 1960). Secondly, several of the languages of South Asia show a clear difference between formal style (usually

equated with the "literary language") and informal or colloquial style. Thus, in Kannada, overlying the dialect differences which correspond to caste and to geography, there is a single formal style which all educated people use in certain situations—in lecturing, in dramatic performances, and in all written composition. See Table 1 for a few comparisons between the formal style, on the one hand, and two colloquial dialects, the Brahmin speech and the middle-caste speech of the Bangalore area, on the other hand.

It seems likely that distinct caste dialects have existed in India for a long period, always remaining similar enough to preserve mutual intelligibility. Yet both the Brahmin and non-Brahmin dialects of modern Kannada show historical changes from the Old Kannada and Medieval Kannada languages. In some respects the two dialects show different changes, but they agree in many changes, as in the loss of medial vowels. To explain the cases of identical change in the two dialects, we may consider three hypotheses: (1) the Brahmin dialect inaugurated the changes, and the non-Brahmin dialect followed suit; (2) the non-Brahmin dialect was the innovator, and the Brahmin followed suit; (3) the two dialects independently developed in the same directions. Putting the possibilities in the form of a more general question, we may ask: In the over-all history of a language, are changes initiated predominantly by the higher social strata or by the lower?

It has been suggested that phonetic change, and perhaps linguistic change in general, are initiated by the upper strata, in order to "maintain a prestige-marking difference" from the lower strata (Joos 1952:229). The lower classes are said to narrow the gap again by imitating their social superiors, who are then forced to innovate once more. Thus language change is explained as a "protracted pursuit of an elite by an envious mass, and consequent 'flight' of the elite" (Fischer 1958:52). For a test of this hypothesis, we may consider the Kannada evidence. It can be shown that the Brahmin dialect does indeed innovate more as regards vocabulary change (Bright 1960). Thus in the middle-caste dialect, "curry" is *yesru*, from Old Kannada *esar* (attested from the 13th century); but the Brahmin form is *huli*, originally meaning "sour, a sour substance," and used to mean "curry" only in recent times. Much vocabulary change involves borrowings from Sanskrit or English, and the Brahmin dialect here often introduces foreign sounds along with the foreign words. Thus the Brahmin dialect introduces /z/ in words like *dazan* from English "dozen," where non-Brahmins say

TABLE 1

	Formal	Brahmin Colloquial	Non-Brahmin Colloquial
"name"	hesaru	hesru	yesru
"man"	manuṣya	maṇṣya	mansa
"friend"	snēhita	snēya	sinēyta
"excuse me"	kṣamisu	kṣemsu	čemsu
"for doing"	māduvudakke	māḍokke	māḍakke
"doesn't do"	māduvudilla	māḍolla	māḍalla
"to a wedding"	maḍuvege	maḍvge	maḍuvke
"in a cart"	baṇḍili	baṇḍiyalli	bandvāgi

dajan. On the other hand, the non-Brahmin dialect shows more sound-change within native vocabulary: cf. non-Brahmin *ālu* "milk," Brahmin *hālu* (Medieval *hāl*, Old Kannada *pāl*); non-Brahmin *gombe* "doll," Brahmin *bombe* (Old Kannada *bombe*). In the realm of grammar, the non-Brahmin dialect again seems to have innovated more, showing for example a locative suffix *-āgi* as against Brahmin and Old Kannada *-alli*. In general, the Brahmin dialect seems to show great innovation on the more conscious levels of linguistic change—those of borrowing and semantic extension—while the non-Brahmin dialect shows greater innovation in the less conscious types of change—those involving phonemic and morphological replacements.

Some evidence is available of a similar pattern in the caste dialects of Tamil. For instance, Old Tamil had a retroflex fricative which may be transcribed /ʒ/; this is preserved in Brahmin dialects, but merges with /y/, /l/, /l/ or zero in most non-Brahmin dialects. Thus Brahmin *kīze* "down" corresponds to *kīye* and *kīle* in several middle-caste dialects, and to *kī* in a Pariah dialect (Bloch 1910:5-7; and my own observations). On the other hand, Brahmin dialects often innovate by adopting loan words, where non-Brahmin dialects preserve native Tamil vocabulary: "water" is Brahmin *tīrtham* or *jalam* (both from Sanskrit), where most non-Brahmin dialects use *tan̄ni* (from older *tan-nīr* "cold water"; Bloch 1910:22). Fuller material on Tamil dialects which is now being published should make the picture much clearer (Zvelebil 1959).

Both in Kannada and in Tamil, it is understandable that Brahmins' familiarity with foreign languages and their more active intellectual life should favor innovation on what I have called the more conscious level. It is less apparent, however, why the Brahmin dialect should be more conservative than others in the less conscious types of change. A possible hypothesis is that literacy, most common among Brahmins, has acted as a brake on change in their dialect—that the "frozen"

phonology and grammar of the literary language have served to retard change in Brahmin speech. A possible test of this hypothesis lies in a consideration of the Tulu society of South India, on the coast west of the Kannada-speaking area. Brahmin and non-Brahmin dialects exist in Tulu, as in other South Indian languages; but there is no established writing system for any form of Tulu, and literacy among the Tulu people exists only for their second languages—Sanskrit, Kannada, and English. Material on the Tulu caste dialects is scanty, but suggestive: the Brahmin and non-Brahmin dialects show phonemic change in approximately equal degree (Bright 1960). When further Tulu data become available, they may give strong support to the hypothesis that although "conscious" linguistic change comes largely from higher social strata, "unconscious" change is natural in all strata where the literacy factor does not intervene.

Finally, we should consider the possible role of social dialects in the process of sound change itself. It has recently been suggested that the locus of phonemic change may be not within individual dialects, but in the process of large-scale borrowing from one dialect to another. "No speaker of English can easily see himself giving up the contrast between, say, *clip* and *lip* . . . Yet that is more or less what happened to *knight* and *night* . . . a few centuries ago." It is hypothesized that some members of the English-speaking community may have pronounced *knight* with, let us say, an unreleased /k/; other speakers, attempting to imitate "the source dialect of their high-prestige neighbors," misheard the /kn/ as /n/ and initiated the new pronunciation, homonymous with *night* (Hoenigswald 1960:55). This hypothesis can be applied to the Kannada material: When Old Kannada *pāl* "milk" became Medieval Kannada *hāl*, the initial /h/ presumably at first retained the voicelessness of its prototype. In modern Kannada, however, the /h/ of Brahmin *hālu* is at least partly voiced. It is possible that this subphonemic change, occurring in the Brahmin dialect, was misapprehended

by non-Brahmins; so that attempting to imitate *hālu* with voiced /h/, they said *ālu* instead. Such an explanation would change the picture previously presented of Brahmin and non-Brahmin roles in linguistic innovation: The upper class would now appear to originate sound change on the phonetic level; the lower class, imitating this inaccurately, produces change on the phonemic level.

Needless to say, we cannot now be certain that such a process operated in any particular historical change. What is possible and highly desirable is that social dialects and their interactions in contemporary societies should be studied in minute detail, bearing hypotheses like the above in mind. South Asia appears to provide an exceptionally rich field for this type of sociolinguistic investigation.

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THE INTERNATIONAL DIRECTORY OF ANTHROPOLOGISTS

A New Committee for a Fourth Edition

THE URGENT NEED for a fourth edition of the *International Directory of Anthropologists* has stimulated the formation of a new (U.S.) National Research Council Committee responsible for producing the volume when funds become available. Alfred Kidder II is Chairman of the Committee, and Lawrence Krader has been appointed Editor.

Although the present *Directory* was issued in 1950 and has become increasingly obsolete, publication of a new edition has been delayed for lack of funds. The American Anthropological Association financed the first *International Directory*, which was published in 1938 by the National Research Council and edited by Alfred V. Kidder. A second edition appeared in 1940. The intervention of World War II prevented publication of a third edition until 1950, when it was issued as a project of the Joint Committee on International Relations in Anthropology, formed after the war by the National Research Council and the American Anthropological Association. Melville J. Herskovits was Editor. Funds for the third edition were contributed by the American Anthropological Association and the Viking Fund. While attempts to finance the fourth edition have so far been unsuccessful, the Committee is more optimistic about its present application for funds.

The preparation and distribution of a comprehensive questionnaire for the *Directory* is another important problem, involving several questions of basic policy: (1) who should be included; (2) what information is most useful; (3) how information can best be obtained; and (4) how to ensure complete coverage of the profession. These problems were encountered by earlier Editors, whose decisions often differed. The first edition, for instance, included many persons, who, although active in anthropology, primarily belonged to other fields, as well as a number of amateurs. The third edition, however, generally excluded these persons and restricted itself to professional anthropologists. The biographical information listed in the first edition included name; institution; date and place of birth; degrees; past positions; membership in societies; anthropological interests; research under way; and field work. To these items the third edition added citizenship (if this differed from country of origin) and geographical areas of interest. Since such differences primarily reflected changing concepts and personnel within anthropology, it seems probable that policies for the fourth edition's questionnaire will also differ.

In his Foreword to the third edition, Herskovits gives a detailed account of some of the practical difficulties met in eliciting information. Besides the difficulty of classifying the variety of academic degrees and positions, he notes that "the greatest difficulty arose out of the good intentions of those who, despite a limited knowledge of English, described their research interests in this language. Sometimes it was possible to translate such statements back into the language of the colleague who had submitted the form, and with this accomplished, it was a simple matter to find the English equivalents. . . . Perhaps the greatest test of ingenuity in drawing the biographies from the forms was presented by the problem of deciphering handwriting. Many hours were spent in attempting to fit meaning to what had been set down. Where it was out of the question to decipher a given item, it was omitted. Much of the inevitable misspelling of local and personal names that will be found in the biographies will have to be read with this factor in mind."

To obtain as complete coverage of the profession as possible in the third edition, the Committee on International Relations in Anthropology enlisted the co-operation of Contributing Editors in every country where anthropologists were active, and consulted anthropological journals. The questionnaire used also included a request for names of other persons who should be listed in the *Directory*.

Although the questionnaire to be used in compiling the fourth edition has not yet been formulated, a tentative proposal for one was drawn up in 1958 by Erminie Wheeler-Voegelin, Chairman of the then Committee (financed by the Wenner-Gren Foundation) to develop the new *Directory*. It is reproduced here as an example of what such a questionnaire might look like.

1. Name
 Family name Personal name
 Date of birth
 Sex: ☐ Male ☐ Female
 Country of birth
 Present citizenship
 Marital Status:
 ☐ Single
 ☐ Married
2. Highest earned academic degree:
 Degree
 Year granted
 University
 Field of specialization
 Other earned degrees and
 advanced professional training:

3. Honors and awards (honorary degrees, medals, prizes, fellowships, lectureships):

4. Current professional activities:
 Teaching ☐
 Research ☐
 Administration ☐
 Student ☐
 Retired ☐
 Other (specify)

- Present position:
 Title
 Institution
 Address
- Other current professional occupation:

- Past positions held
 (list chronologically, with dates):

5. Membership in professional organizations:
(give names of societies and membership status—e.g., fellow, life member, etc.)
6. Publications: (list no more than three; give title, date and place of publication, or journal references)

Offices held in professional organizations:

7. Major anthropological interests:

(a)	(b)	(c)
Check your field or fields of specialization	Give a brief title description of your specific interest(s) in each field indicated in column (a)—e.g., human paleontology, social anthropology, structural linguistics.	Indicate geographical areas to which the interests indicated in columns (a) and (b) apply. List continents, countries, or smaller areas, according to the scope of your interests.
Physical		
Anthropology <input type="checkbox"/>		
Archaeology <input type="checkbox"/>		
Ethnology <input type="checkbox"/>		
Folklore <input type="checkbox"/>		
Linguistics <input type="checkbox"/>		

8. Field work. Give name of group and/or locality, and dates:

9. Current mailing address:

10. Please list below any persons you know who should be included in the **International Directory of Anthropologists**, and whom we may have missed in our mailing-of questionnaires:

Name Mailing address

Charcoal Carbon-14 Date for Folsom

The first charcoal-derived radiocarbon date for a Folsom site has been announced by George A. Agogino of the University of Wyoming and C. V. Haynes of Golden, Colorado (U.S.A.). Three grams of charcoal from the Lindenmeier site in Colorado were dated at $10,780 \pm 375$ years B.P. by Isotopes, Inc. The investigators acted as agents for the Smithsonian Institution in this research, and were supported by 1959 and 1960 grants from the American Academy of Arts and Sciences. This date, the first from a Folsom "camp-site," confirms the ages previously indicated for Folsom and obtained from less reliable material or from levels more recent than the Folsom occupation.

Research Plans

► Anti-Marriage Movement in China

I am working on an article concerning a female anti-marriage movement that took place in Shuntak, Kwangtung, China, from about the middle of the nineteenth century until the late 1930's. It was based on a number of uncoordinated associations consisting of spinsters and married women; many of them were lesbians. Their object was not marriage reform, but the protection of members from outside attempts to make them marry or undertake the duties of marriage, and also to maintain certain standards of conduct among themselves. I shall first describe the movement by putting down as systematically as I can all the recorded details plus some extra information from my own informants. I shall then analyse the conditions under which the movement emerged, and discuss the probable reasons it took the form it did. Finally, I shall examine some of the factors that might have led to its breakdown.

MARJORIE TOPLEY

► Macacus Irus

I am now working on the dissection and measurement of the *macacus irus* in order to determine its growth pattern, variations within the species, and its comparative anatomy. If I obtain research funds, I shall undertake a two-year study of the mongolian spot on new-born babies in Japan, which has become much more difficult to find since World War II.

EHARA AKIYOSHI

Research Reports

Rural Taiwan: Second Socio-Economic Survey

The First Socio-Economic Survey of Rural Taiwan, generally referred to as the Raper Report, was conducted in 1952 and published in 1953 as *Rural Taiwan: Problem and Promise*. In 1959 a second survey was undertaken, one of its more direct purposes being to repeat and follow up the first, to identify and measure the changes and trends of the intervening seven years and establish a good practical model with which to repeat the same exercise periodically in the future. The Second Survey concerned the same sixteen areas selected as representative by its predecessor, but it adds two areas in which industrialization and the introduction of new basic crops, with urbanization and concomitant changes, have freshly taken place during the intervening period. This was a comprehensive random-sample survey by questionnaire and interview, carried out by the Joint Commission for Rural Reconstruction; students and staff members of the National Taiwan University; and the Department of Economics and Political Science, University of Hong Kong.

Official statements formally representing its conclusions will be published later by the Survey; personal notes and formulations for a working-report follow.

The rapid process of "modernization" in Taiwan has greatly accelerated in the last six years. The traditional pattern of the rural Chinese family, for instance, is disappearing, and tendencies in this area that were already evident seven years ago are now spreading rapidly. The influence of the family on modes of life and thought has markedly declined; the family unit has become smaller, and relationships are simpler. At present, the most immediate and powerful mechanism in the simplification of family structure is the tendency for married brothers to separate and establish their own conjugal families at an earlier age.

This is a superimposition on the much longer established and slower (although recently accelerated) trend toward over-all weakening of family authority at the big-family, patriarchal level. Of the three generations—grandparental, parental, and minor—the most strongly felt separation divides the first and second. The third remains fairly strongly attached to the second, but on the new basis of smaller conjugal households; and the grandparental generation, which still cherishes the ancient pattern of the extended family, dislikes this situation.

Clan and institutional links are far from weakened in the parental generation, but take on new group and functional forms, conflicting with the position and attitude of the elders. "Beyond the family, in the community"—as Professor Martin M. C. Yang has well expressed it—"the *Gemeinschaft* traditions are giving way to relations of a *Gesellschaft*."

Although the breakdown of social control is quite a serious problem in Taiwan today, most persons see this as transitory. The main reasons for this belief are: (1) confidence in the basic capacity of the Chinese people, and their power of adaptation to new conditions; (2) the great growth of education (school attendance was 98% in 1959), and its increasing modernization; (3) aggravation of the present situation by two temporary factors: the recent development of urbanization and its special stress in certain areas; and the relatively large numbers of ex-servicemen seeking settlement in recent years.

Males now marry at an average age that is five or six years higher than it was six years ago. Four reasons are given for this: (1) couples defer marriage until the young man has completed his compulsory military service; (2) many young people are continuing their studies beyond Middle School, and deferring marriage for this reason; (3) fear of having too many children before they can be properly supported; and (4) less insistence by older generations on having grandchildren as soon as possible.

The last six years have also seen a marked shift of community leadership from persons of 60-70 to a distinctly younger age-group of 30-50. The latter are more efficient at meeting modern organizational needs, better trained for committee work, and more practical-minded. Political leadership is thought to change too frequently, but the administrative functions and personnel seem much more stable and are more efficient.

Much of the American and European missionary effort formerly directed at China is now concentrated upon Taiwan. Its impact is most marked on the aboriginal mountain people, who find ritualistic or evangelistic denominations particularly attractive. Proselytizing is also extensive among the Chinese lowlanders, but in a less emotional strain. Yet the indigenous religions have not lost ground absolutely, but instead are sharing in the current expansion of religious interest.

The tide of urbanization, including the growth of cities and townships as well as the more general changes in social and personal values, goals, and outlooks, is very strong. In some areas

industrialization has caused local seasonal shortages of farm labor. Growth of markets and market orientation is universal. The great rise in consumption, commodity-availability, urbanization, and market contacts is causing general changes in living patterns and attitudes. There is much pressure for more secondary education. The propensity to consume is high, and expresses itself largely in neo-urban patterns of expenditure and behavior. Above all else, there is consciousness of the population problem, particularly because of the shortage of good cropland, which is exacerbated by its increasing use for building and military purposes. In line with the general increase in "realistic" thinking, personal and family attitudes toward population problems have changed profoundly, emphasizing matters of everyday life rather than more remote issues. The farmers who earlier considered ten children per family the optimum now stipulate five or six children if the family is reasonably well-off, and fewer if there is hardship. Limitation of the size of families is increasing. A majority of the leaders of local opinion openly favor birth-control, but complain of the difficulty of obtaining information, and the cost of contraceptives. Conservative rural opinion fears that the better-educated people will limit their progeny, while others will not; and that the quality of the nation will decline. Only in the newly industrialized and still industrializing areas is there much conviction that industrialization can possibly be the sole solution.

Reported by E. STUART KIRBY

A New Fossil Anthropoid from the Miocene of Austria

The relatively large number of fossil anthropoids of Miocene age that have been found in the Republic of Austria and its immediate neighborhood includes *Sivapithecus* (?) *darwini* ABEL (Abel 1902) and *Pliopithecus* (*Pliopithecus*) *antiquus* BLAINV. (Glaessner 1931) from the Tortonian of Neudorf an der March in the Vienna Basin; *Pliopithecus* (*Epipliopithecus*) *vindobonensis* ZAPFE & HUEZELER (Zapfe 1958) from the Helvetian fissure deposit of Neudorf an der March, Czechoslovakia; and *Austriacopithecus* *weinfurteri* EHRENBURG (Ehrenberg 1938) from the Tortonian of Klein Hadersdorf in Lower Austria. Many remains of the dentition of *Pl.* (*Pliopithecus*) *antiquus* BLAINV. have been found in the Tortonian lignites of Goerlach in Styria (Hofmann 1893); and teeth of *Dryopithecus* *fontani carinthiacus* MOTTI (Mottl 1957) were recently discovered in the Upper

Miocene (Sarmatian) lignites of St. Stefan im Lavanttal, Carinthia.

To these discoveries may now be added a new find, from the Middle Miocene (Tortonian) lignites at the Trimmelkam coal mines in Upper Austria on the Salzach river north of Salzburg. This find includes the anterior part of the left half of a mandible, with both premolars and the first molar. Only a clay cast is preserved of the canine, which was rather large and indicates that this individual was a male. A middle and a lateral incisor were lying isolated.

The teeth are morphologically similar to those of *Pl. (Pliopithecus) antiquus* and *Pl. (Epipliopithecus) vindobonensis*, but are distinctly smaller in size (length of the premolars and the first molar is 15.5 mm.), and show certain differences in form: the outline of the first molar is longer and smaller; its mesial part (Trigonid) is markedly smaller than the distal part (Talonid); and the pit of the Trigonid is closed in its mesial part. The very distinctive sectorial structure of the anterior premolar, although different from *Pl. (Pliopithecus) antiquus*, is quite similar to *Pl. (Epipliopithecus) vindobonensis* and also resembles *Limnopithecus*, in which the pit of the Trigonid of the posterior premolar is closed in a similar way. The morphological differences from *Limnopithecus*, of the East African Miocene, are relatively less important than those between *Pl. (Pliopithecus) antiquus* and *Limnopithecus* (Le Gros Clark and Leakey 1951; Le Gros Clark and Thomas 1951). Undoubtedly, the new Primate is closely related to *Pliopithecus*, but is not identical with any known species.

This new find indicates the existence in the European Miocene of a rather large group of small anthropoids, more or less closely related to *Pl. (Pliopithecus) antiquus*. With our knowledge of fossil anthropoids so incomplete, the author feels it necessary to indicate this relation in the nomenclature, and has established only a new subgenus (*Plesiopliopithecus*) and a new species (*lockeri*). The full name of the new Primate, therefore, is *Pliopithecus (Plesiopliopithecus n. subgen.) lockeri n. sp.* A final decision about the systematic condition of the *Pliopithecus* group will not be possible until the skeletal characters of the different subgenera and species are known. So far as can be inferred from *Pl. (Epipliopithecus) vindobonensis* and *Limnopithecus macinnesi*, these small anthropomorphs were generalized and rather primitive in the structure of their skeletons. In the proportions of the humerus, *Limnopithecus macinnesi* seems to have advanced further in the direction of the

modern brachiators. *Pliopithecus* has often been considered a gibbon. Huerzeler (Hürzeler 1954) regarded phylogenetic relations between *Pliopithecus* and the hylobates as possible, while Le Gros Clark and Thomas thought the genus *Limnopithecus* "may . . . legitimately be termed a primitive Gibbon" (1951:26). Undoubtedly the origin of the recent Hylobatidae may be found in the *Pliopithecus* group or in the closely related genus *Limnopithecus*, which Simonetta, in fact, unites with *Pliopithecus* (1957:59). At present, *Limnopithecus* seems more closely related to the recent Hylobatidae, in spite of the well-argued opposite opinion recently presented by Ferembach (1958).

The new discovery of *Pl. (Plesiopliopithecus) lockeri* demonstrates that the extent of variation within the *Pliopithecus* group as a whole seems not yet to have been exhausted. The systematic search for fossil Primates in Central Europe gives hope of further interesting results.

Reported by HELMUTH ZAPFE

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An Early Site in Highland Ecuador

From January 23 to February 7, 1960, a two-man party from the University of Oklahoma Museum, Norman, Okla., U.S.A., carried out preliminary investigations in the Ecuadorian Andes near Quito at an archeological site that had been reported to us earlier by Allen Graffham, a geologist of Ardmore, Oklahoma. We collected a large number of obsidian and basalt artifacts from the surface of El Inga, a badly eroded hill-slope flanking Ilalo Mountain near the town of Tumbaco.

Obsidian artifact types found include several styles of projectile points, side and end scrapers, ovate blades, graters, drills, prismatic blades, microblades, and small hemispheric polyhedral cores. Two projectile point styles appear from field observation to be most significant: The dominant style is a large stemmed point identical in form with the points that characterize Level I at Fell's Cave in Chile (Bird 1938); one of the several minority styles is a lanceolate form like the North American Clovis point, but it has been found only in fragmentary form. Both these styles are characterized by basal and edge grinding, and have irregular channel flakes removed from either or both faces. They are, in fact, "fluted" points of two distinct forms.

Basalt artifacts are generally irregular; but well-made scrapers, choppers, "pulping planes," and *manos*, as well as used flakes and cores, have been recovered.

Two test excavations made in an un-eroded portion of the site demonstrated that the artifacts are contained in a layer of dark soil, 12 to 15 inches thick, which comprises the soil mantle at the site. These tests clearly demonstrate the feasibility of extensive excavation at El Inga.

At El Inga there seem to be many typological elements previously unknown in South America. The Fell's Cave type stemmed point seems the best chronological marker; in Chile, this is associated with extinct sloth and horse, and has been dated by carbon-14 at 6900 B.C. The presence of the fluting technique and channel flake removal, as well as the lanceolate form, suggest early North American types. The presence of blades and microblades is rare in South America, and probably is another indication of relationship with early North American technology.

Samples of the obsidian have been submitted to the Smithsonian Institution obsidian-dating project. All frag-

ments indicate age of the order of 10,000 years, but definitive samples must await a major excavation.

El Inga is interpreted as the camp and workshop site of some of the earliest inhabitants of South America. These were probably part of the first major wave of migrants from North America, and would thus pre-date the Fell's Cave people by at least a thousand years. By this interpretation, it is quite likely that early North American styles and traits would be most prominent in northwestern South America.

A report on this preliminary project is being prepared and major excavations are planned for 1961.

Reported by WILLIAM J. MAYER-OAKES
and ROBERT E. BELL

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Institutions

► The INTERNATIONAL LIBRARY OF AFRICAN MUSIC at Msaho, Union of South Africa, was founded in 1954 by the African Music Society to continue the work of the African Music Research organisation, and to collect and classify representative selections of music from as many language groups as possible. No other organisation has attempted this in Africa on so large a scale. Following its primary objective of publishing as much of its collection as possible, the Library has assembled 100 long-playing 12" records, which it is publishing under the title of "AMA" records ("The Sound of Africa Series"). Each disc contains about 40 minutes of recorded sound. The series, over three-fourths of which is now available for distribution, includes examples of authentic music, stories, and songs from more than forty tribes of southern Africa, covering the Congo, Federation of Rhodesia and Nyasaland, Mozambique, Union of South Africa, Swaziland, and Basutoland. While a few special selections will be pressed for general release, such issues represent only a small proportion of the Library's total output, and the bulk of its recordings are available only to members. The annual fee for ordinary members is £ 5, which helps cover the cost of collecting, classifying, editing, and printing; and the records, which can only be issued in limited editions, cost an additional £ 2.15.0 each. The Library is not in a position to lend records from its limited edition issues, but welcomes visitors and students at its

headquarters, Msaho, Roodepoort, near Johannesburg. For terms upon which recordings will be made available to educational and scientific bodies, a special memorandum can be obtained from the Library, P.O. Box 138, Roodepoort, Transvaal, South Africa.

► The SOCIETY FOR THE HISTORY OF TECHNOLOGY was founded in 1958 as the first systematic attempt in the U.S. to encourage studies of the development of technology and its relations with society and culture. To meet the need of an organization and a scholarly periodical specifically devoted to the study of technology and society, formerly touched on by existing societies and journals only occasionally and as a peripheral interest, a group of scholars from many different fields established the Society. Its interests concern not only the history of technology, but also the relations of technology to science, politics, social change, the arts and humanities, and economics. The Society holds meetings to investigate the problems connected with the impact of technology on society, sponsors programs in conjunction with other organizations, and publishes a quarterly journal, *Technology and Culture*, which includes articles of general interest dealing with the development of technology and its relations with society and culture, as well as material concerned with the history of technological devices and processes. Subscription to the journal is included in the membership fee, which also entitles members to participate in meetings and other activities of the Society, and gives them the opportunity to support its aims, programs, and publications. Membership is open to individuals, organizations, corporations, and institutions interested in its purposes and activities. The fee for regular members is U.S.A. \$8.00. Further information about the Society or the journal may be obtained by writing the Secretary: Dr. Melvin Kranzberg, Room 311, Main Building, Case Institute of Technology, Cleveland 6, Ohio, U.S.A.

► A new HONG KONG BRANCH of the ROYAL ASIATIC SOCIETY has recently been inaugurated, and plans to publish, in an annual journal, material about Chinese history and language and data on the immigrant Chinese and other ethnic groups in Hong Kong. Further information may be obtained from Marjorie Topley, Rural Heritage, Taipo Kau, New Territories, Hong Kong.

► The ANTHROPOLOGICAL SOCIETY OF YUGOSLAVIA was founded in May, 1959, to promote research on physical an-

thropology as well as teaching of the subject in Yugoslav universities. Correspondence may be addressed to the President of the Society, Prof. B. Škerlj, University of Ljubljana, or to its Secretary, Dr. Ž. Gavrilović, Institut za medicinska istraživanja, Bulevar JNA 18, Beograd, Yugoslavia.

Serial Publications

► *A Field Guide to the Ethnological Study of Child Life*, by Sister M. Inez Hilger, College of St. Benedict, St. Joseph, Minnesota, U.S.A., has been published by the HUMAN RELATIONS AREA FILES as the first volume in a new series, the *Behavior Science Field Guides*. Part of HRAF's continuing program to facilitate and advance research in the fields related to the study of man, the new series, which may be used with the *Outline of World Cultures* and *Outline of Cultural Materials* (CURRENT ANTHROPOLOGY, volume 1, no. 3, p. 256), are intended as guides for organizing field research. Later volumes in the series will extend to other aspects of human behavior and culture.

Since the Guides are intended as practical aids to field work, it is expected that they will achieve final form only as they are tested in the course of actual research by other scholars. Comments and criticism by the users, therefore, will be welcomed by both the author and publisher. The 70-page Guide is available for the U.S.A. \$1.25 from the Human Relations Area Files, 421 Humphrey Street, New Haven, Conn., U.S.A.

(Continued on page 440)

Aboriginal Languages of Latin America

The Table in the six pages that follow was prepared by the Editor (Sol Tax) for the *Encyclopædia Britannica*, from which it is reproduced, by permission, to make it readily available for criticism, correction, and use by scholars.

Please see the note at the end of the table, p. 436.

Classified Aboriginal Languages of Latin America—Continued
After McQuown (1955) and Greenberg (1956)*

Phylum	Stock	Family	Subfamily	Languages
Macro-Oto-Manguean —Continued	Mixtecan	Mixteco		Mixteco, Cuicateco, Amusco (Mexico)
	Chinanteco			Chinanteco (Mexico)
	Zapoteco	Zapoteco		Zapoteco (Serrano Norte), Zapoteco (Serrano Sur), Zapoteco (Valle) (Mexico)
	Chatinoan	Chatinoan		Chatino, Papabuco, Solteco (Mexico)
Tarascan				Tarasco (Mexico)
		Miskitoan		Baham, Cabo, Mam (Honduras), Miskito, Taurin, Wanki (Nicaragua; Honduras?); Nicaraguan unless otherwise shown)
		Sumoan	Uluian	Cuena, Guanexco (Honduras; Nicaragua?), Pinzo, Ulva, Yoso (Nicaragua unless otherwise shown)
	Mismalayan		Sumo	Baúlaca, Baa, Carabula, Caca, Hausshane, Jacu, Panamaca, Pisq, Sumo?, Taulhac, Tunqui (indicates Honduras and Nicaragua; those not marked are Nicaraguan alone)
			Unclassified	Cu (Nicaragua; Honduras?), Musutepes, Silam, Yasca (Nicaragua unless otherwise shown)
		Matagalpan		Cacaopera (El Salvador), Chato, Dule, Matagalpa (Honduras; Nicaragua?), Pantasma (Honduras unless otherwise shown)
		Unclassified		Banham, Cuahay, Kivalha, Sumo-Sirpe, Tungha (Nicaragua)
		Pacific-Chibchan	Isthmian	Burica (Costa Rica, Panama), Chaltva, Changuena, Chiru, Dorasco, Duy, Escoria, Move, Muñi, Murie, Nata, Penomenyo, Valente (Panama unless otherwise shown)
Chibchan		Talamanca	Colombian	Ahurra, Jamundi, Lile, Timba, Yameci, Yolo (Colombia)
				Boruca, Briuri (Costa Rica, Panama), Buruaca, Cabecar, Chiripo, Coto, Estrella, Guatar, Pocosi, Quepo, Quilla (Colombia), Surree, Tarica, Telti (Costa Rica, Panama), Terraba (Panama), Tucurique, Turucaca, Yolo (Costa Rica unless otherwise shown)
		Interandean-Barbacoan	Barbacoa	Cará, Cagaya, Gadoquer, Cocora (Nicaragua), Colima, Colaradot, Cucua (Panama), Cuna, Paya (Panama), Golané (Costa Rica), Gualuso (Costa Rica), Muelanides, Nigau, Palia, Sindagua (indicates Ecuador; those not marked are Colombian)
			Paezan-Cocomicoan-Popayanens	Cocomico, Guamaco, Mogues, Paez, Paniquita, Polindara, Popayan, Pubenaro, Purace, Totoro (Colombia)
Macro-Chibchan		Eastern Chibchan	Cundinamarcan	Acata, Chibcha (Muisca), Duit, Sinsiga, Tuncho (Colombia)
			Arhuaco	Atanque, Bintiaca, Cuaaba, Guamaca, Ica, Sanha (Colombia)
			Central American Chibchan	Melchora, Rama (Nicaragua)
			Northern Yuncan	Cayari, Maushita, Purlia (Ecuador)
			Southern Yuncan	Machica, Quingnam (Peru)
		Yuncan		Manta, Puna, Tumbica (Ecuador)
		Atlan	Caraquean	Caraque, Huancavilba (Ecuador)
			a	Gayon (Venezuela)
			b	Ayoman (Venezuela)
		Xuguan		Xagua, Culba (Venezuela)
	Jirajaran		d	Jirajara (Venezuela)
			a	Mariusa (Venezuela)
			b	Chaguan (Venezuela)
	Guaraunan		c	Gualqueri (Venezuela)
			d	Guarau (Venezuela)
Chirihan		Guahariban		Chiriana (Brazil), Guaharibo, Brazil, Venezuela, Uaiwa (Brazil, Venezuela)
			b	Carime (Brazil)
Murán			c	Puracau (Brazil)
			a	Mura, Matanaui (Brazil)
			b	Piraba (Brazil)
			c	Jabahi (Brazil)
Unclassified				Aikame (Ecuador), Chono (Brazil), Bonama (Bolivia), Lenca (Honduras, El Salvador), Xinca (Guatemala)

Classified Aboriginal Languages of Latin America—Continued

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Conferences

Seminar on Economic Types in Pre-Urban Cultures of Temperate Woodland, Arid, and Tropical Areas

October–December, 1958, at the Chicago Natural History Museum, Chicago, Ill., U.S.A.

Sponsored by The Wenner-Gren Foundation for Anthropological Research, Inc.

Organizing Chairmen: Joseph R. Caldwell, Illinois State Museum, Springfield, Ill., U.S.A.; and Creighton Gabel, Northwestern University, Evanston, Ill., U.S.A.

Regular participants:

ROBERT J. BRAIDWOOD, University of Chicago.

J. DESMOND CLARK, The Rhodes-Livingstone Museum, Livingstone, Northern Rhodesia.

MELVIN L. FOWLER, Southern Illinois University, Carbondale, Ill., U.S.A.

F. CLARK HOWELL, University of Chicago.

ARTHUR J. JELINEK, University of Chicago.

GEORGE I. QUIMBY, Chicago Natural History Museum.

CHARLES A. REED, University of Illinois, Urbana, Ill., U.S.A.

PHILIP WAGNER, University of Chicago.

Other participants:

ROBERT M. ADAMS, University of Chicago.

JAMES B. GRIFFIN, University of Michigan, Ann Arbor, Mich., U.S.A.

PAUL MARTIN, Chicago Natural History Museum.

JOHN RINALDO, Chicago Natural History Museum.

WALTER W. TAYLOR, Southern Illinois University.

GORDON R. WILLEY, Harvard University, Cambridge, Mass., U.S.A.

HOWARD WINTERS, Southern Illinois University.

Discussion:

The primary purpose of the seminar was to examine the prehistoric evidence in several archaeologically and environmentally distinct zones in order to isolate, if possible, some of the cultural processes and ecological factors promoting or retarding cultural change. Consideration was limited largely to technological and economic matters, particularly subsistence patterns, since archaeology undeniably lends itself most readily to this sort of interpretation. With more sophisticated methods of assessing the economic bases, descriptions of prehistoric communities as a whole should prove more definitive

than they have in the past. In light of the vast amount of material involved, it was agreed that the seminar should concentrate on data ranging from advanced hunting and gathering groups of the late Pleistocene up to the point when urban settlement and/or historical records first appeared in any given region. During the sessions, a series of papers and informal presentations were given on Southwestern Asia, Southwestern United States, Eastern United States, Northwestern Europe, Sub-Saharan Africa, and Latin America. These provided much of the framework for subsequent discussions, giving us a cross-section of subsistence types and environments which we hoped would shed some light on the respective roles of each in channeling cultural change. General interest was expressed in the alternative modes of subsistence characterizing different times and places, and some attempts were made to define subsistence types along lines similar to those sketched by Braidwood (Braidwood and Reed 1957; Braidwood 1960). More specific considerations centered about agricultural transitions—both (1) indigenous domestication of plants and animals and (2) the impact of food-producing economies in non-nuclear areas with respect to such things as cultural resistance and selectivity or the ecological settings of the contact zones. Another recurrent topic of discussion concerned postglacial climatic changes and the role these played in altering economic adjustments.

Results:

Within the category of food-collection, one finds a range of variation from near-random gathering to intensive and sometimes highly selective collection. Settlement patterns run the gamut from nomadic to sedentary, dependent on both the resources of the habitat and the technological proficiency of exploitation. Certainly in view of the Northwest Coast of North America, and of Caldwell's (1958) interpretation of Adena and Hopewell as non-agricultural, there seems little doubt that some food-collectors attained considerable socio-economic complexity (even though their subsistence economies may have been ecologically restricted), and their accomplishments force us to re-evaluate some of our assumptions about the respective potential of food-collection and food-production. It proves rather difficult to generalize about this. Technology is our major source of information, and we have tended to overemphasize it in some respects. Productivity, which relates to both exploitative technology and ecological circumstances, must be taken into account if we are to explain

why some types of food-collection can be as efficient as many forms of agriculture.

Cultural responses to postglacial climatic changes are most clearly documented in the more diversified collecting economies of the northern temperate zone (Mesolithic, Archaic, Desert Base). Whether the transition to agriculture in Southwest Asia at this same time represents an alternative economic adjustment to changing climatic conditions is more dubious in terms of the natural-historical evidence.

One of the primary concerns about food-production was the question of agricultural incipience—where and why? Recognition of this phase of subsistence lies largely in the hands of natural scientists, since the settlement patterns and artifact assemblages would not have altered enough to be recognizable archaeologically. Although far from clarified in detail, Southwest Asia appears the most likely contender as a "zone of natural habitat" and a nuclear area of agricultural development. Other hypotheses concerning independent origins of agriculture, in Southeast Asia (Sauer 1952), the Sudan (Murdock 1959: 64–70), or Ethiopia (Vavilov 1949–50), are based upon inference rather than any kind of archaeological fact. Until such a time as we have information to the contrary, it must be admitted that these tropical forms of cultivation (including those of northeastern South America) can be more satisfactorily explained in terms of culture contact, with agricultural concepts being transferred to local plants. Somewhat disturbingly for those who have looked toward the heart of Nuclear America for the origins of maize cultivation, the highest radiocarbon dates for plant domestication in the Western Hemisphere have been obtained from the semi-arid fringes to the north, in Tamaulipas (MacNeish 1958) and New Mexico (Dick 1954). Some agricultural incipience, as in the instance of Mississippi Valley seed cultivation (Fowler 1957; Willey 1960:79), may represent little more than the extension of food-collecting techniques from a functional point of view.

Something might be learned of the basis for agricultural innovation, or contrasts in receptivity to agricultural technology, through comparison of the subsistence types and habitats in which these changes took place. It was suggested, for example, that more generalized collectors (e.g., Zarzian, Natufian) probably were those most likely to have initiated domestication, while more selective collectors (Magdalenian, Nootka) were less susceptible to change in this direction. Gathering populations, as in the Southwestern U.S. Des-

ert Base, or more sedentary collectors, like Mesolithic or Archaic midden-dwellers, were perhaps more inclined to indulge in plant cultivation than were hunters, who conversely might have found animal husbandry more congenial. Familiarity with growth cycles exhibited by food-collectors, as in controlled flooding and irrigation by Great Basin Indians (Forde 1950:35) or burning-off of land to promote wild-plant yields in the American Northwest (French 1957), give ethnographical hints of the cultural-ecological background leading to practical cultivation.

There are a number of suggestions that food storage may have stimulated cultural change by allowing more effective use of food supplies, especially in season-bound situations. Although the developmental potential might well be different, such things as permanency of settlement and population density would be effected with the storage of any food staple, whether wheat or acorns. It is conceivable that plant and animal domestication initially represented an extension or improvement of food-storage techniques by the first agricultural experimenters.

Some question exists whether one can speak of a "Neolithic Revolution" in Childe's terms. The concept, while generally valid, may be partly erroneous, for food-production apparently need not always result in an economic revolution. This seems particularly relevant if we think of seed cultivation on Mississippi shell heaps, or even of tropical horticulture.

Since we had agreed not to pursue subsistence types beyond the threshold of urbanization, no concentrated effort was made to isolate the factors responsible for urban growth. It became apparent, however, that this aspect of cultural change and development may prove difficult to explain purely on the basis of subsistence patterns and ecology (e.g., intensive irrigation farming in the river valleys of semi-arid regions) and without taking into full account altered social, political, and economic patterns, which are probably at least as significant.

In the light of some of these considerations, the archaeologist must be especially cautious when he is obliged to identify subsistence type through examination of artifact assemblages, settlement features, or mortuary practices alone. This consideration leads in turn to the problem of measuring extractive efficiency, since it has become obvious that one cannot make sufficiently definitive statements about subsistence patterns simply by extrapolation from remnants of material culture. We are in need of some reasonably accurate means of estimating productive capacities that

is independent of whether food was obtained by hunting and gathering or by farming. The short list of technological traits that occur only in association with agricultural economies (prepared by Meighan *et al.* 1958:144) demonstrates the difficulty of judging subsistence from indirect sources, because virtually all these items occur only in advanced agricultural contexts. This is, of course, an extreme example which takes every exception into account, but it does serve to show us where we stand, particularly with respect to early food-producing horizons. A good cross-section of floral and faunal materials is indisputably the best method of determining exactly which of the available resources were utilized, and which made up the largest share of the diet, or whether agriculturally-produced foods were used; and theoretically one can carry this a step further and make some comments on the relation of diet and extractive technology to nutrition and population density (Bartholomew and Birdsall 1953; Deevey 1956; Braidwood and Reed 1957).

A delineation of subsistence-settlement types, while useful as a tool for grasping the cultural and ecological inducements for prehistoric cultural change is made exasperatingly difficult by a number of factors. Part of the difficulty is inherent in our data, which are at best only partially complete; but neglect in obtaining all the evidence and indifference to reconstructing activities from the total evidence are not insurmountable obstacles. Ecological and chronological variability, the problems of treating mixed economies, and the attempt to emphasize cultural processes as well as culture types contribute further to our dilemma. Yet it is clear that no unilinear scheme will suffice, since chronological differences and cultural alternatives must be included in the framework.

In summary, we must study environments as backgrounds against which technological and economic changes take place. At the same time, these changes have to be seen in terms of both innovation and historical contact, so that we ask ourselves, on one hand, why certain food-collectors began to experiment with plant and animal domestication in Western Asia or Latin America; and on the other hand, why intrusive agricultural economies did not immediately replace hunting and gathering in Europe and the U.S. once they were introduced. Furthermore, we must leave room for consideration of cultural traditions which, even though the archaeologist scarcely can reconstruct them, could easily alter the course of events. An excellent example of this is given by Taylor, who in his Coahuila

excavations finds an almost unbelievable degree of conservatism in food-collecting techniques and even basketry or sandals styles over a period of 10,000 years. To meet the analytic and interpretative problems that are becoming increasingly important in archaeology, we will have to seek aid from every quarter—ethnology, history, geography, geology, botany, zoology, meteorology—as well as sharpen our own perceptions concerning our goals.

CREIGHTON GABEL

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SERIAL PUBLICATIONS

(Continued from page 430)

► The *Plains Anthropologist*, journal of the PLAINS CONFERENCE, has resumed publication with issue number 9, May, 1960. The PLAINS CONFERENCE is an informal organization of archaeologists, ethnologists, linguists, physical anthropologists, and other scientists whose research interests center in the Plains area of the U.S.A. and Canada, and the bordering tiers of states and provinces. It has met annually since 1931.

The journal, published quarterly, is devoted to the anthropological interpretation of the Plains area. Inaugurated in 1954, it is the successor to the *Plains Conference Newsletter*, begun in 1947. Recently re-organized editorially, the journal includes representation from all the Plains states and the disciplines of anthropology. It runs approximately 48 pages per issue.

Subscription rates remain U.S.A. \$2.00 for the year 1960. All correspondence with the journal should be addressed to JAMES B. SCHAEFFER, Editor, *Plains Anthropologist*, Research Institute, North Campus, University of Oklahoma, Norman, Oklahoma.

Terminology

► "Primitive" Peoples

Like many other anthropologists, I have used the term *primitive* with increasing misgivings. A textbook published in 1958, called *A Profile of Primitive Cultures* (Service), includes descriptions of the Arunta, Yahgan, Andamanese, Eskimo, Tungus, Cheyenne, Nuer, Navaho, Jivaro, Nootka, Trobrianders, Tahitians, Kalinga, Maya, Inca, Ashanti, and villages in Yucatan, Morocco, China, and India. Here, and in earlier books like *Our Primitive Contemporaries* (Murdock 1934), the term seems to mean only "peoples that anthropologists study." Then, why *primitive*?

This is not only a matter of semantics. In recent travels, it became evident to me that our tolerance of this term will be harmful to anthropology. An anthropologist is defined as "one who studies primitive peoples"; but by the dictionary meanings of *primitive*, there are no such groups—or, at least, the peoples that we study are not primitive as they understand the term. In the world emerging today, scholars who study "primitive" groups are out of subject-matter.

I undertook to write a paper on this subject for the Central States Anthropological Society's meeting in Bloomington, Indiana (U.S.A.), in May, 1960, and asked a research assistant, Lois Mednick, to prepare some material on uses of the term *primitive* in Anthropology. Her memorandum is reproduced here for its general interest.

Meanwhile, it became evident to me that the reason we have never found an acceptable substitute—such as *preliterate*, *native*, etc.—is that the anthropological use of *primitive* is not a legitimate concept deserving a single term. We lump together all of the peoples of the world, past and present, except those which are part of Western Civilization and its ancient progenitors. Anthropologists, of all people, should know that this is not a legitimate class. If an oak tree should demand a term for all other trees, leaving out only oaks, should one be surprised if no suitable answer can be found? The truth is that all these peoples we call *primitive* have nothing in common that is not also shared by all human societies and cultures. Such a quality would have to be something (1) characterizing equally Andamanese, the Arunta, Ashanti, Cheyenne, Chinese, Eskimo, Inca, Indians, Jivaro, Kalinga, ancient and modern Maya, Moroccans, Navaho, Nootka, Nuer, Tahitians, Trobrianders, Tungus, and Yahgan; and (2) distin-

guishing all of these from Ikhnaton's Egypt, Pericles' Athens, Cellini's Florence, Shakespeare's England, Napoleon's Europe. We have not discovered any such quality; rather, we have concluded that there is no such thing. But we carry on with a word for the first group because they are the peoples who became our special subjects for study (the second group was already well-known).

Although any single term would be inappropriate, the one we happened to choose—*primitive*—is peculiarly so, because of its connotations. It is a curious irony that when cultural relativists abandoned the invidious terms, Savage, Barbarian, and Civilized, they applied the even more invidious misnomer *primitive* to those whom the evolutionists had called savages and barbarians, and in addition used it for those "civilized" peoples not well-known to Europeans. Anthropologists tend to use *primitive* less often in theoretical contexts (where, of course, it has no genuine referent) than in titles of courses and of books, and in ordinary parlance, where—greatest irony of all—our influence is greatest! We put the term in quotes, and we know it is not true; but can others be blamed for saying that, in the division of labor, anthropologists are the persons who study primitives?

SOL TAX

Memorandum on the Use of *Primitive*

Webster's unabridged Dictionary lists the following usages for the word *primitive* [F. *primitif*, fr. L. *primitivus*, fr. *primus* first]:

- (1) Of or pertaining to the beginning or origin; earliest in time; first; original; as *primitive* ages or religion; our *primitive* ancestors.
- (2) Of or pertaining to the earliest ages, as of man or civilization, or the earliest period, as of Christianity or an art; as, *primitive* tools or morals; the *primitive* church; *primitive* lack of perspective.
- (3) Original; primary; radical; not derived; as, a *primitive* verb in grammar.
- (4) Characterized by a quality or qualities belonging or ascribed to the original state of man, an institution, etc., as naturalness, mildness, simplicity, etc.; as, to live in *primitive* fashion.
- A really *primitive* people nowhere exists. McLennan.
- (5) Of or pertaining to the denomination or denominations of Primitive Baptists, Methodists, etc.
- Originally, perhaps, members of some *primitive* sect, they were now in the natural course of things members of the Church of England. Galsworthy.
- (6) Biol. a Primordial. b Ancient, archaic, little evolved, persistent;—said of species or groups whose structure more or less closely approximates that of their early ancestral types; as, the opossums are *primitive* mammals; the tuatara is a *primitive* type of reptile, etc. Cf. PROMORPH.
- (7) Geol. Earliest formed; fundamental;—

applies esp. to the crystalline rocks of the Archean.

Syn.—First, pristine; aboriginal, ancient, antiquated. See PRIMARY.

Ant.—Modern, recent, contemporary, present.

A search of present-day anthropological literature soon makes it clear that the use of *primitive* by anthropologists does not accord with its dictionary definition. Several recent books whose titles include the word *primitive* class together under this term (1) such diverse societies as those of Philippine headhunters and Hindu peasants (Service 1958); (2) the laws of the Indians of the North American Plains, and those of the Ashanti (Hoebel 1954); (3) the religion of the Murngin and that of the people of Dahomey (Goode 1951). In *Primitive Heritage* (1953), Mead and Calas include articles ranging from Aztec human-sacrifice to Pawnee war-parties. The selections in these four volumes appear to have been chosen for contrast of material and diversity of locale; the only common denominator uniting the various cultures and societies seems to be the fact that all of them have been studied by anthropologists.

In *The Primitive World and its Transformations* (1953), Redfield draws on contemporary data in order to understand what cultures were like before the rise of *literati* and cities. He uses the term *primitive* as synonymous with *preliterate* (p. xi), but his "primitives" range through time, encompassing peoples who were primitive in the etymological sense of the word and peoples who recently have become literate.

Not only is it hard to define *primitive* as the term is used by anthropologists, but sometimes an author who uses the word in a book title scarcely refers to the concept in his text. The series of essays and lectures by Radcliffe-Brown collected in *Structure and Function in Primitive Society* (1952) is a case in point. His essay, "Primitive Law," makes no use of the word in the entire exposition; and the eleven remaining articles use the word only four times—twice in one address to a lay audience.

The illusiveness of *primitive* is attested to by the many authors who feel it necessary to explain or justify the manner in which they have used the word.

I have used here throughout the term 'primitives' without further explanation. I hope this has not conveyed the impression that I consider these tribes as living in an original state of nature, such as Rousseau imagined. On the contrary, we must remember that every primitive people has had a long history. It may be descended by decay from a stage of higher development or it may have arisen to its present state battling against vicissitudes. There is no primitive tribe that is not hemmed in by conventional laws and customs. The more primitive it is the

greater is the number of restrictions that determine every action (Boas 1888; reprinted in Boas 1948:633).

... They are not old. But they are nevertheless primitive. The point to be grasped is that among contemporary societies primitiveness does not necessarily mean antiquity, in spite of the fact that primary means first. What it does mean is that the cultural forms of primitive societies are more similar in their general characteristics to those that presumably prevailed in the early cultures of the infancy of mankind (Hobbel 1954:289-90).

'Primitive,' to be sure, is not a good word for the peoples investigated by social anthropologists, though we use it because it has become customary. Primitive societies are not always primitive in the sense of 'simple' ... Primitives are better called nonliterate; but besides their failure to develop written languages, they have another characteristic in common. In these societies, more activities are carried on by organizations, membership in which is determined by kinship, than in any modern Western Society (Homans 1950:192).

Leslie White (1949:241) has still another definition to add:

... Primitive society: tribes based upon kinship ties, free access to the resources of nature for all, relatively little social differentiation and specialization, and a high degree of social equality.

These quotations exemplify a few of the different meanings that social and cultural anthropologists attach to the word *primitive*. Actually, however, each of the five sub-fields of anthropology (archaeology, linguistics, physical anthropology, social anthropology, and ethnology) has redefined *primitive* in terms of its own interests and needs, in much the same way as has been done with the word *culture*. This is perfectly satisfactory, if there is agreement within each field as to what the word means. Unfortunately, this is not the case.

For example, one linguist (Greenberg 1953:265) talks about "primitive" languages and means unwritten languages; while Beals and Hoijer (1954: 508-11), in their discussion of primitive languages, also put the word in quotation marks and then proceed to demonstrate the fact that there are no such languages.

In archaeology, Caso (1953: 228, 230) equates his Middle American Primitive Horizon with the Old World Neolithic; Movius (1953:189) defines it differently:

For during prehistoric times, just as in the historic range, environmental factors played a dominant role in conditioning the behavior of various early (or primitive) groups of mankind.

In physical anthropology, the situation is just as confused. Simpson (1950: 101), adhering to a strict biological definition, uses *primitive* to mean "slowly evolved," contrasting it with advanced, or rapidly evolved, organs or

organisms. Washburn (1944:70) declines to use the word and warns that it "changes its meaning constantly." Three quotations from Hooton (1954) demonstrate the aptness of Washburn's warning:

In [whites] the presumably primitive wavy hair form predominates (p. 581).

In the Iron-Age, an Upper Paleolithic or Combe-Capelle type of primitive Mediterranean penetrated into South India (p. 615).

The pygmies are scattered in small groups through the Congo, have no language of their own, and are usually attached as hunters or pets to the full-sized agricultural negroes in their districts. They know no agriculture, have no domesticated animals and are generally in about as primitive a state as can be imagined (p. 623).

An examination of the literature of ethnology and social anthropology adds to the growing confusion. Twenty-three different meanings for *primitive*, some used on combination, and others frequently standing alone, were compiled during a few hours search. Most often, *primitive* means:

Noncivilized (Calas 1953: xxvi; Coon 1948: v; Firth 1951: v, 80; Kroeber 1953: xiii; Mead 1953: xviii).

Simple (Homans 1950:192; Radcliffe-Brown 1952:153).

Small (Fortes and Evans-Pritchard 1950:5; Nadel 1951:52).

Pre-literate or Non-literate (Homans 1950:192; Mead 1953: xvii; *Notes and Queries* 1954:28; Radcliffe-Brown 1952:3; Redfield 1953: xi).

Non-Western (Coon 1948: vi; Firth 1951:10; Herskovits 1940:237, 246, 261, 264).

Static (Firth 1951:80-81; Malinowski 1936:80).

Tradition-bound (Boas 1948:633; Malinowski 1936:80).

Some of the less frequently encountered meanings are:

Exotic (Kroeber 1953: xiii).

Unprogressive (Firth 1951:80).

Backward (Radcliffe-Brown 1952:2).

Tribal (White 1949:241, 377).

Isolated (Redfield 1953: xi).

Homogeneous (Redfield 1953: xi).

Nonmonetary (Firth 1951:133-34).

Uncritical, Prescientific (Bidney 1953: 332, 334).

Pseudo-scientific (Malinowski 1936: 80).

This does not exhaust the usages, but to list more, or to cite other than representative sources, would prolong this memorandum needlessly. The point should be clear. The use of *primitive* is ambiguous and inconsistent within each separate field of anthropology, and between the fields of anthropology; above all, one wonders what meaning *primitive* has when the larger public is addressed.

Unfortunately, confusion in the use of *primitive* does not stop at the threshold of anthropology, since any scholar from any discipline is free to borrow the already amorphous concept for his own use. Freud (1913) makes use of it in these ways:

Another trait in the attitude of primitive races towards their rulers recalls a mechanism which is universally present in mental disturbances, and is openly revealed in the so-called delusions of persecution (p. 807).

Primitive man is known to us by the stages of development through which he has passed ... moreover, in a certain sense he is still our contemporary: there are people whom we consider more closely related to primitive man than ourselves, in whom we therefore recognize the direct descendants and representatives of earlier man. We can thus judge the so-called savage and semi-savage races; their psychic life assumes a particular interest for us, for we can recognize in their psychic life a well-preserved, early stage of our own development ... a comparison of the psychology of primitive races as taught by folklore, with the psychology of the neurotic as it has come to be known through psychoanalysis, will reveal numerous points of correspondence and throw new light on subjects that are more or less familiar to us (p. 846).

In fairness, it is interesting to compare this second quotation from Freud with an anthropological work written forty years later: Margaret Mead (Tax et al. 1953:174) makes the following statement:

The other problem that Dr. Caudill raises, which is important at the primitive and the ethnological level (and we will not be able to get information out of already accumulated material), is the one raised by all the psychiatrists as to whether there is some kind of deeper layer in man that is unaffected by culture. There is supposed to be something going on inside, sometimes called the 'unconscious' or the 'deeper impulses,' that is equated by psychiatry everywhere with a human nature invariant, unaffected by culture. We probably can solve this important problem far more quickly with primitive than with civilized material.

Jung (1929), in *The Psychology of the Unconscious*, makes this statement:

Primitive thinking and feeling are exclusively concretistic; they are always related to sensation. The thought of primitive man has no detached independence but clings to the material phenomena. The most he can do is raise it to the level of analogy. Primitive feeling is always equally related to the material phenomenon ... The primitive does not experience the idea of divinity as a subjective content, but the sacred tree is the habitat—nay even the deity himself (p. 534).

Additional references in this same vein could be cited; but the case should not be overstated, for several more recent writers show a more modern tenor in their use of anthropological data. Erikson (1950) writes:

Finally we turn to cultural primitiveness

as the apparent infancy of humanity where people seem, to us, to be at one moment as naive as children, at another, as possessed as lunatics. . . . We now know that primitives have their own adult normality, that they have their own brands of neurosis and psychosis and, most important, that they too have their own varieties of childhood (p. 95).

The discovery of primitive child-training systems makes it clear that primitive societies are neither infantile stages of mankind nor arrested deviations from the proud progressive norms which we represent: they are a complete form of mature human living often of a homogeneity and simple integrity which we at times might well envy (p. 96).

However, later in the same book, one reads:

The system underlying Sioux education is a primitive one—i.e., it is based on the adaptation of a highly ethnocentric, relatively small group of people, who consider only themselves to be relevant mankind (p. 139).

Turing from psychiatry to economic history, we find the following quotations in Polanyi (1944):

To start with, we must discard some nineteenth century prejudices that underlay Adam Smith's hypothesis about primitive man's alleged predilection for gainful occupations. Since his axiom was much more relevant to the immediate future than to the dim past, it induced in his followers a strange attitude towards man's early history. On the face of it, the evidence seemed to indicate that primitive man, far from having a capitalistic psychology, had, in effect, a communistic one (later this also proved to be mistaken) (p. 44).

Developing the topic, "The motive gain is not natural to man," Polanyi (p. 45-46) cites the following anthropological sources:

The characteristic feature of primitive economics is the absence of any desire to make profits from production of exchange (Thurnwald 1932). Another notion which must be exploded, once and forever, is that of the Primitive Economic Man of some current economic textbooks (Malinowski 1930). We must reject the *idealtypes* of Manchester liberalism which are not only theoretically, but also historically misleading (Brinkmann 1924).

In concluding the section on primitive economics, Polanyi states:

Primitive economics as studied in the preceding pages is not far distinguished from any other form of economics, as far as human relations are concerned, and rests on the same general principles of social life (p. 53).

Here is an example of how a legal historian, William Seagel (1941:33-35) uses *primitive*. Under the fourth chapter heading on primitive law, entitled, "Custom is King," he writes that primitive societies are lawless and live under an "automatic sway of custom" that seems to suffice. Therefore, "there is no law until there are courts." And if people who are ordinarily recognized as

primitive have courts, then *ipso facto* they are not primitive, for "to speak of the law of some African peoples as 'primitive' although they have courts and have invented many complex forms of legal transactions which compare not unfavorably with those of the ancient Babylonians is to abuse the natural meaning of the term."

Two examples from philosophy use *primitive* to denote peoples with a different emotional and mental outlook from that of modern man:

. . . A high degree of civilization was always first necessary for the animal man to begin to make those much more primitive distinctions of 'intentional,' 'negligent,' 'accidental,' 'responsibility,' and their contraries, and apply them in the assessing of punishment. That idea—"the wrong-doer deserves punishment because he might have acted otherwise," in spite of the fact that it is nowadays so cheap, obvious, natural, and inevitable, and that it has had to serve as an illustration of the way in which the sentiment of justice appeared on earth, is in point of fact an exceedingly late, and even refined form of human judgment and inference; the placing of this idea back at the beginning of the world is simply a clumsy violation of the principles of primitive psychology (Nietzsche 1929 ed.:19).

Primitive man expresses his feelings and emotions not in mere abstract symbols but in a concrete immediate way. We may speak of a tension between stabilization and evolution. . . . In myth and in primitive religion the tendency to stabilization is so strong that it entirely outweighs the opposite pole. Mythical thought is, by its origin and by its principle, traditional thought. . . . From the point of view of primitive thought the slightest alteration in the established scheme of things is disastrous. . . . Primitive religion can therefore leave no room for any freedom of individual thought (Cassirer 1944:280-82).

Interestingly enough, the sociologist Mannheim (1960:542), commenting on Weber, writes:

Max Weber observed that even in primitive communities the psycho-pathological types usually become the prophets, saviors, and reformers, changing the old ways of life and breaking down the old magical attitudes. In his view, this is because in societies whose customs are sanctified mainly by magic it is the psychopath who is unadjusted and who therefore dares to break these old habits, which are no longer fitted to the changed situations, and is able to discover new and better adjusted attitudes.

A final example is the classic *Primitivism* of Rousseau, which Muller (1957:51) characterizes as follows:

. . . men looked to primitive societies to discover human nature in its 'pure' state. In fact, however, we cannot find such a universal state of nature. We find instead a fantastic diversity in design for living, with often an outrageous disregard of the reputed laws of human nature. And this commonplace of modern anthropology is pertinent because first of all we have to deal with primitivism—the familiar idea that civilization is a disease, and that the only possible cure for it is to return to

nature and the simple life. The myth of the noble savage is at least as old as Homer . . . while later Greek and Roman literature is full of homilies on the happier, more virtuous life of rude peoples. . . . Primitivism is based on a misunderstanding of primitive life, whose apparent simplicity is complicated by rigid taboos and black magic. The primitivists cherish an impossible desire. They wish to appreciate the simple life with all the self-consciousness and sensitivity that only civilization makes possible; they wish to be children of nature and also poets and philosophers celebrating nature.

It is now quite clear why *Notes and Queries on Anthropology* (1954) abandoned use of the word *primitive* with this statement:

The word native is used in this volume because it is presumed that investigations will be made in the vast majority of cases among peoples in their native habitat. The terms primitive, savage, and aboriginal are avoided because these words have all been used too loosely. It would be difficult to define what any author means by primitive (p. 28).

But after demonstrating the confusion beclouding the word *primitive*, one still wonders how anthropologists departed so far from its dictionary meaning. In the earliest anthropological literature, one finds the concept of primitive a little clearer. Boucher de Perthes (1846) entitled his treatise on prehistoric implements *sur l'Industrie primitive*, obviously meaning that these were the tools of the first men. Likewise, McLennan's first essay on *Primitive Marriage* (1865) was an attempt to reconstruct the early development of mankind on the basis of cultural survivals. McLennan and the other practicing anthropologists of that period were primarily caught up in the formation of what was to become the British Evolutionist School.

Most of those actively engaged in the anthropological arguments of the day rather consistently used *primitive* in the sense of "primeval" or "original"; and most of the confusion over the meaning of the word seems to have stemmed from the newness of the discipline. Concepts were being formed, definitions were being hammered out, data were being garnered, and there was as yet no world-wide communication among scholars. Under these circumstances, it is surprising that there is not more disagreement in the literature.

In *Primeval Man*, the Duke of Argyll (1872) writes:

It has not, however, been sufficiently observed that the inquiry into the Primitive Condition of Mankind resolves itself into three separate questions. . . . First, the Origin of Man considered simply as a Species . . . second, the Antiquity of Man. . . . Third, His Mental, Moral, and Intellectual Condition when first created (p. 24-25).

. . . . The creature "not worthy to be

called a man," to whom Sir J. Lubbock has referred as the progenitor of Man, was, *ex hypothesi*, deficient in those mental capacities which now distinguish the lowest of the human race (p. 67).

Sir J. Lubbock speaks of Primeval Man as having been in a condition of "utter barbarism" . . . it affords no presumption whatever that barbarism was the Primeval Condition of Man, any more than the traces of Feudalism in the laws of modern Europe prove that feudal principles were born with the Human Race (p. 131-33).

Sir J. Lubbock's theory is, that in these Savages we see something rather above than under the Primitive Condition of Mankind (p. 69-70).

Sir J. Lubbock seems to admit that this loathsome practice was not primeval, probably because he considers it as unnatural. . . . Cannibalism is only an extreme case of a general law, and it is a crucial test of the fallacy of a whole class of arguments commonly assumed by those who support the Savage-theory respecting the Primeval Condition of Mankind (p. 134-36).

And here it is important to observe that even if savage races be taken as the type of man's Primeval Condition, the evidence afforded by those races is all in favour of the conclusion that as regards his characteristic mental powers, Man has always been Man, and nothing less (p. 150).

The Duke of Argyll precedes the classic formulation of the Evolutionist theory and nomenclature. With the publication of Tylor's *Anthropology* in 1888, most of the early ambiguities were dispelled for the main ranks of British anthropologists. For Tylor, *primitive* meant exactly what the root denoted; he used it to refer to that long-gone original population of mankind about whom one could only speculate. In his descriptions and characterizations of living and historical populations and their cultures, Tylor used the familiar, and now almost discarded, hierarchy of Savage, Barbarian, and Civilized.

Going back as far as philology can take us, we find already existing a number of language groups, differing in words and structure, and if they ever had any relationship with one another no longer showing it by signs clear enough for our skill to make out. Of an original primitive language of mankind, the most patient research has found no traces (p. 12).

No competent anatomist who has examined the bodily structure of these apes considers it possible that man can be descended from any of them, but according to the doctrine of descent they appear as the nearest existing offshoots from the same primitive stock whence man also came (p. 40).

. . . the Andaman tribes may be a remnant of a very early human stock, perhaps the best representatives of the primitive negro type which has since altered in various points in its spread over its wide districts of the world (p. 88-89).

The original tongue whence these are all descended may be called the Primitive Aryan (p. 156).

The primitive shield was probably the parrying shield (p. 222).

But though there was a consistent use of *primitive* by the British Evolutionists, one cannot deny that the literature

peripheral to the main currents of anthropological thought contained the loose, ill-defined, use of the word. The first volume of the *Journal of the Royal Anthropological Institute* (1871), for example, included an article by C. S. Wake entitled "The Mental Characteristics of Primitive Man as Exemplified by the Australian Aborigines," in which he used a slightly different interpretation of *primitive*:

I wish, primarily, to establish what are the real mental phenomena exhibited by the natives of Australia; and secondarily, to show approximately the conditions in which man generally must have existed in the primeval ages, not necessarily when he first appeared on earth, but so soon as the struggle for existence between man and man commenced, and the selfish instincts of humanity had had time to become fully developed (p. 74).

Also, the early American, French, German, and Italian journals of this period—1871-1900—reveal a growing tendency to use the word *primitive* loosely. The *Bulletin de la Société d'Anthropologie de Paris* (1875) contains two articles: D. Eichthal's "Sur le texte primitif du premier récit de la création," and Abbé Petitot's "Sur la contemporanéité de la pierre taillée, de la pierre polie, du bronze et du fer chez les peuplades primitives de la Scandinavie, de la Bretagne, et des Gaules." Although the German journals of this period do not seem to use the words *primitive* or *Primitivität*, they abound with such words as: *ursprünglich*, *altertümlich*, *Urmensch*, *Urzeit*, and the like, all of which the English translations render as "primitive," "primitive man," "primitive era," etc. Since the German prefix *Ur-* means "primeval" or "primitive," the suspicion might be entertained that the German usage is in the Tylor tradition.

The Italian journals also list a variety of articles that use *primitive* in their titles; but for the most part, these are reports from travelers or missionaries, or represent peripheral researches, most of which are by persons with some special point of view—e.g., Caesar Lombroso's article (1896), "L'uomo primitivo e l'atavismo."

The first volume of the *American Anthropologist* (1888) includes an article by J. W. Powell entitled "From Barbarism to Civilization," which shows the influence of the evolutionists in its title, if not in its total content:

. . . another set of writers have discovered among such peoples only evidence of primitive innocence and the happiness of primitive simplicity, and such people have been pictured as angels (p. 99).

It is doubtful, however, whether we can trace our current difficulties either directly or solely to such sources as

these, which for the most part represent fringe interests of anthropological theory. Instead, it seems that our difficulties stem from the demise of the Evolutionist School. It appears that in our anxiety to scuttle unilinear evolutionary theory, we also felt it necessary to disavow the use of Savage, Barbarian, and Civilized. This left us with no term, then current, other than the loose, non-evolutionist usage of *primitive*. Thus, *primitive* at first became the blanket word for savage and barbarian. Today, however, now that anthropologists have also studied Mexican, Mayan, Andean, Hindu, and Chinese cultures, these are also called *primitive*, even though anthropologists would be the first to uphold their status as civilizations.

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Offered

► I have an extensive photographic documentation of "peg-calendars" from peoples in many parts of the world (e.g., Siberia, Aleutians, Alaska, East Greenland, Venezuela, Surinam, Congo, South Africa, Philippines, Indonesia), largely assembled from museum collections. These are plaques of wood or other material (bone, horn, ivory) with seven or thirty holes, into which a peg is inserted progressively (or through which a cord or thong is drawn) to keep track of the days of the (presumably European) week or month. Apparently disseminated by explorers, missionaries, and traders, these are now rare in Europe itself, where the device can be traced back at least to classical antiquity (*parapegmata*), and seems to have survived in common use as late as the 17th century. The curiously wide diffusion of such calendars thus poses an interesting problem in acculturation and culture history. It is perhaps only in West Africa that such calendars appear to form part of an indigenous culture (Bernard Maupoil, "La géomancie à l'ancienne Côte des Esclaves, *Travaux et Mémoires de l'Institut d'Ethnologie de l'Université de Paris*, vol. 42, 1943, pp. 209 ff.). Since I shall not work up the material myself, I should like to make it available to a scholar or specialist who may be interested in it. In the meantime, I would be glad to add any documentation that readers of CURRENT ANTHROPOLOGY may care to contribute. CARL SCHUSTER, R.D. Box 416, Woodstock, New York, U.S.A.

► "Ancient Cultures of Siberia and the Problem of the First Relations of the Old World with the New World," an article by A. P. Okladnikov printed in *Revista de Cultura*, Universidad Mayor de San Simon, Vol. III, Cochabamba, Bolivia, 1958, has been translated into English and a limited number of copies run off. Apparently more recent than the article recently translated and published at Harvard University, this article reviews Siberian archaeology and discusses relations with America. Copies are available on request from GEORGE F. CARTER, Department of Geography, Johns Hopkins University, Baltimore 18, Maryland, U.S.A.

► The "Sixth Report on Palynology in Africa" for the years 1958-59, compiled with the aid of 70 colleagues, gives summaries of palaeoclimatology, pollen morphology, aerobiology, and pollen analyses carried out in different parts of Africa. Close co-operation among archaeologists, geologists, and botanists is stressed. A limited number of copies

is available on request for those interested in the report.—E. M. VAN ZINDEREN BAKKER, Palynological Research, Department of Botany, University of the Orange Free State, Bloemfontein, South Africa.

Wanted

► A copy (reprint, or the separate issue of the journal) of Julius Lips' "Fallensystemer der Naturvölker," published in *Ethnologica*, vol. 3, 1927. Kindly notify DR. F. BOURLIERE, 15 Avenue de Tourville, Paris VI, France.

► . . . Reprints of anthropological publications for the use of students and others at Panjab University, Chandigarh, India, which is shortly going to start a department of anthropology. We are particularly handicapped for want of literature on paleo-anthropology, growth studies, and animal—especially primate—behavior; but reprints of articles on other aspects of anthropology are also welcome. All Associates are asked to send such reprints to: DR. S. R. K. CHOPRA, University Reader in Anthropology, Panjab University, Chandigarh, India.

► . . . Reprints of all kinds of anthropological and sociological articles, for the DEPARTMENT OF ANTHROPOLOGY OF THE UNIVERSITY OF INDONESIA. The Department, which is at the early stage of building up an anthropological library, will very much appreciate receiving reprints from all Associates of CURRENT ANTHROPOLOGY who are interested in Indonesia and want to establish an exchange relationship.—KOENTJARANINGRAT, Department of Anthropology, University of Indonesia, Djalan Diponegoro 82, Djakarta, Indonesia.

► . . . To know how many scholars would buy an English translation of *Die Mbowamb*, Vicedom and Tischner's monumental three-volume study of a Highland people of New Guinea. Originally published in German, it is now out-of-print. The work is important for the ethnography of New Guinea and for comparative anthropological studies. It would be expensive to publish a translation—£ A 15.15.0 fully-bound, £ A 8.8.0 mimeographed—but the Department of Anthropology and Sociology and the Publications Committee of The Australian National University will consider publication if there is sufficient interest.—J. A. BARNES, Department of Anthropology and Sociology, The Australian National University, Box 4, G.P.O., Canberra, A.C.T., Australia.

PUBLICATIONS RECEIVED

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See Volume I, Number 2, for an explanation of the entries and a key to the abbreviations used.

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Amer. studies, anth. theory, ling. Mar-
tinique, West Indies, Fr. Union. S-F

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Plantation soc., family, underdeveloped
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dian langs. of Mesoamer. A-N-S

J

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Ling. Gen., descriptive & hist. ling. esp.
theory of phonological change, methods of

reconstruction, proof of genetic relation-
ships, hist. of ling. Amer. Indian langs.,
Indo-Eur. langs. esp. Italic, Germanic,
Indic; Dravidian Langs.

K

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& pers., soc. patterns of psychopath.

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pol. struct. & insts., comp. high cul. study.
Mesoamer. & neighboring areas, Tibet. N-M-A

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40, N.Y., U.S.A. Librarian, New York Pub-
lic Libr. Prehist. & classical arch. Neo-
lithic & Bronze Age of Central & E. Eur.,
hist. of costume (prehist. & ancient),
probs. of domestication. Eur. E

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1960). Arch., gen. anth. Arch. reporting,
arch. method & theory, late Pleist. geol.,
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World. N-E

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(mammal) remains from arch. excavations
in Poland. Central Eur., Poland. E

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L

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Groups within western civ.

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Tales, legends, folk-belief of Japanese peo-
ple in comp. with neighboring peoples.
Japan & Far East. A

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Mixe ling., beliefs, customs, folklore. Folk
med. & witchcraft, ethnobotany & ethno-
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some comp. data from neighboring tribes,
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O

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Study of cul. relat. in Bronze & Iron Age
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See Volume I, Number 2, for explanation of the entries and a key to the abbreviations used.

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AMERICAN ANTHROPOLOGICAL ASSOCIATION, 1530 P. St. N.W., Washington 5, D.C., U.S.A. Gen. anth.

ANTHROPOLOGIAI KÖZLEMÉNYEK, Csalogány-u 45b. IV. 2, Budapest II, Hungary. Phys. anth.

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CENTRES D'ÉTUDES ANTHROPOTECHNIQUES, 42, R. Henri Barbusse, Paris 5, France. The application of anth. to sports, med., subjects of work.

CENTRO DE INVESTIGACIONES ANTHROPOLOGICAS DE MEXICO, Madero 27-211, Mexico 1, D.F., Mexico. All branches of anth.

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E

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ETNOGRAFISKA MUSEET, Norra Hamngatan 12, Göteborg C., Sweden. Ethnography of Amer. & S.E. Asia.

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INTERNATIONAL AFRICAN INSTITUTE, St. Dunstan's Chambers, 10/11 Fetter Lane, Fleet St., London E.C. 4, England. Afr. ethnol., sociol., ling.

INTERNATIONAL LIBRARY OF AFRICAN MUSIC, P.O.B. 138, Roodepoort, Transvaal, South Africa. Recording & issuing Afr. music on gramophone discs.

INTERNATIONAL SOCIETY FOR GENERAL SEMANTICS, 400 W. North Ave., Chicago 10, Ill., U.S.A. (c/o S. I. Hayakawa, 225 Eldridge Ave., Mill Valley, Calif.). Semantics.

ISTITUTO DI GEOLOGIA E MINERALOGIA, Via Boldini 14, Ferrara, Italy. Human paleon. & palethnol.

J

JOURNAL OF ASIAN STUDIES, Northwestern University, 1808 Chicago Ave., Evanston, Ill., U.S.A. Soc. scis. & humanities (Asia).

JOURNAL OF AUSTRONESIAN STUDIES, 1701 Beach Dr., Victoria, B.C., Canada. All aspects of Austronesian studies.

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LABORATOIRE D'ANTHROPOLOGIE, Faculté des Sciences, 2 R. du Thabor, Rennes (Ille-et-Vilaine), France. Phys. anth., prehist. & protohist.

LABORATOIRE D'ANTHROPOLOGIE SOCIALE, 19, av. d'Iena, Paris 16, France. Soc. anth. (kinship, relig., myth), rural studies.

LOS ANGELES CITY COLLEGE, Earth Science Dept., 855 N. Vermont Ave., Los Angeles 27, Calif., U.S.A. Gen., phys. & cul. anth., Calif. arch.

M

MICHIGAN STATE UNIVERSITY, Dept. of Sociology & Anthropology, East Lansing, Mich., U.S.A. Soc. anth., arch., applied anth.

MUSÉE ROYAL DU CONGO BELGE, Tervuren, Belgium. Phys. anth., ethnol., soc. & cul. anth., ling.

MUSEO ARQUEOLÓGICO DE LA SERENA, Casilla 117, La Serena, Chile. Arch., phys. anth., ethnol., S. Amer. culs.

*MUSEO DE CIENCIAS NATURALES, Ministerio de Educacion Nacional, Caracas, Venezuela.

MUSEO ETNOGRAFICO, Moreno 350, Buenos Aires, Argentina. Ethnogr. & cul. theory.

MUSEO NACIONAL DE ANTROPOLOGÍA Y ARQUEOLOGÍA, Pueblo Libre, Lima, Peru. Arch. of Peru.

MUSEO VICTOR EMILIO ESTRADA, P.O.B. 798, Guayaquil, Ecuador. Arch. of Ecuador.

*MUSEUM FÜR VÖLKERKUNDE, Neue Hofburg, Heldenplatz, Wien I, Austria.

MUSEUM FÜR VÖLKERKUNDE UND VORGESCHICHTE, BIBLIOTHEK, Binderstr. 14, Hamburg 13, Germany. Ethnol. of Amer., Afr., Oceania, Central & N. Amer., gen. ethnol.

MUSEUM OF NEW MEXICO, Dept. of Anthropology, P.O.B. 1727, Santa Fe, N.M., U.S.A. Anth., arch. of N. & S. Amer., ethnol. of N. Amer.

MUSEUM OF NORTHERN ARIZONA (Northern Arizona Society of Science and Art, Inc.), P.O.B. 402, Flagstaff, Ariz., U.S.A. Arch. & ethnol. of S.W. U.S.A., esp. Hopi & Navajo.

MUSEUM VOOR FOLKLORE, Gildekamersstr. 2-6, Antwerpen, Belgium. Flemish folk art.

MUSEUM VOOR LAND- EN VOLKENKUNDE, Willemsskade 25, Rotterdam, Netherlands. Cul. anth., of Afr., Amer., Asia, Indonesia & Oceania.

N

NANZAN UNIVERSITY ANTHROPOLOGICAL INSTITUTE, 6, Goken'ya-cho, Showa-ku, Nagoya, Japan. Arch. excavations, ethnol. rscs. & ling. study.

*NATIONAL MUSEUM, Dept. of Ethnographical Collections, 10 Ny Vestergade, Copenhagen, Denmark.

NATIONAL MUSEUM, P.O.B. 266, Bloemfontein, South Africa. Anth. & arch.

NATIONAL TAIWAN UNIVERSITY, Dept. of Archaeology & Anthropology, Taipei, Taiwan, Republic of China. Arch., Ethnol. of China & S.E. Asia.

NATURHISTORISCHES MUSEUM, Anthropologische Abteilung, Burggraben 7, Wien I, Austria. Phys. anth.

NEBRASKA STATE HISTORICAL SOCIETY, 1500 R. St., Lincoln 8, Nebr., U.S.A. Arch. & ethnol. of Nebr. & Central Plains area.

*NORSK FOLKEMUSEUM, Bygdoy Oslo, Oslo, Norway.

*NUFIELD BLOOD GROUP CENTRE, Royal Anthropological Institute, 21, Bedford Square, London W.C. 1, England.

O

OESTERREICHISCHE ETHNOLOGISCHE EXPEDITIONS- UND FORSCHUNGSGESELLSCHAFT, Reitschulgasse 2, Vienna I, Austria. Ethnol.

P

PITT RIVERS MUSEUM, University of Oxford, Oxford, England. Teaching Dept. in ethnol. & prehist.

R

*RESEARCH INSTITUTE FOR THE STUDY OF MAN, 162 E. 78th St., New York 21, N.Y., U.S.A.

RESEARCH INSTITUTE OF THE SCHOOL OF SOCIAL SCIENCES, Kalevantie 4, Tampere, Finland, (until Oct. 31, 1960: Arkadiankatu 20 B, Helsinki). Sociol., soc. psychol., ecol., & soc. anth., recently studies of values & soc. struct., pop. & soc. participation.

RIJKSUNIVERSITEIT TE GENT, Seminarie voor Archaeologie, Universiteitstr. 16, Gent, Belgium. Eur. prehist., provincial-Roman arch.

RÖMISCH-GERMANISCHE KOMMISSION des Deutschen Archäologischen Instituts, Palmengartenstr. 10-12, Frankfurt a. M., Germany. Prehist., provincial-Roman arch., early hist.

ROYAL ANTHROPOLOGICAL INSTITUTE OF GREAT BRITAIN AND IRELAND, 21, Bedford Square, London, W.C. 1, England. Phys., cul. & soc. anth. & arch., prehist.

S

ST. LOUIS UNIVERSITY, Dept. of Sociology & Anthropology, 221 No. Grand Ave., St. Louis 3, Mo., U.S.A. Phys. anth., arch., soc. anth.

SEATTLE ANTHROPOLOGICAL SOCIETY, Dept. of Anthropology, University of Washington, Seattle 5, Wash., U.S.A. All fields of anth.

SEMINAR FÜR AFRIKANISCHE SPRACHEN UND KULTUREN, Alstergras 3, Hamburg 36, Germany. Afr. langs.

SEMINAR FÜR UR- UND FRÜHGESCHICHTE, Kurze Geismarstr. 40, (20b) Göttingen, Germany. Arch. of Medieval & Dark Ages (Central & N. Eur., Viking arch.), Iron Age, arch. of hunting, food collecting & early food-producing pops.

SOCIEDAD ARQUEOLÓGICA DE LA SERENA, Casilla 125, La Serena, Chile. Arch. of Chile.

SOCIETY FOR AMERICAN ARCHAEOLOGY, c/o AAA, 1530 P. St., N.W., Washington 5, D.C., U.S.A. Arch. of the New World.

SOCIETY FOR APPLIED ANTHROPOLOGY, N.Y. State School of Industrial and Labor Re-

lations, Cornell University, Ithaca, N.Y., U.S.A. Soc. anth., psychiat., health, psychol.

SOUTHWEST ARCHEOLOGICAL CENTER, Gila Pueblo, Box 1562, Globe, Ariz., U.S.A. Arch., archeo-ornithology, archeo-mammalogy, ethnol., ruins stabilization.

STAATLICHES MUSEUM FÜR VÖLKERKUNDE, Täubchenweg 2, Leipzig C 1, German Democratic Republic. Ethnogr.

STAATLICHES MUSEUM FÜR VÖLKERKUNDE, Maximiliansstr. 42, München 22, Germany. Ethnol. & anth. collections, esp. of non-Eur. art.

SUMMER INSTITUTE OF LINGUISTICS, C.P. 4216, Rio de Janeiro, Brazil. Descriptive, comp. & applied ling.

SUMMER INSTITUTE OF LINGUISTICS, Aptdo. 74, Guatemala C.A. Linguistics.

T

TENNESSEE FOLKLORE SOCIETY, George Peabody College for Teachers, Nashville 5, Tenn., U.S.A. Folklore (lit., music, crafts, lang., mores in gen.).

THE CANADIAN RESEARCH CENTER FOR ANTHROPOLOGY, Oblate Ave., Ottawa, Ont., Canada. Human scis., esp. Amerindian anth.

THE FAR-EASTERN PREHISTORY ASSOCIATION, Dept. of Anthropology, University of Arizona, Tucson, Ariz., U.S.A. Far-Eastern arch. & associated subjects.

*THE JAPANESE SOCIETY OF ETHNOLOGY, 132 Shimohoya, Hoyamachi, Kitatama-Gun, Tokyo, Japan.

THE JOS MUSEUM, Dept. of Antiquities, Jos, Nigeria. Prehist. arch., ethnol.

TOKYO METROPOLITAN UNIVERSITY, Dept. of Sociology & Anthropology, 591, Fusumacho, Meguro-ku, Tokyo, Japan. Soc. & cul. anth.

TULANE UNIVERSITY, NEWCOMB COLLEGE, Dept. of Sociology & Anthropology, New Orleans, La., U.S.A.

U

UNIVERSIDAD DE ANTIOQUIA, Instituto de Antropología, Aptdos.: aéreo 1226 y Nal. 229, Calle 48 No. 42-17 Medellín, Colombia. Gen. anth., arch.

UNIVERSITÄT BERN, Seminar für Urgeschichte, Helvetiaplatz 5, Bern, Switzerland.

UNIVERSITÄT BONN, Seminar für Völkerkunde, Liebfrauenweg 7, Bonn, Germany. Ethnol. of S. Amer.

*UNIVERSITÄT HAMBURG, Seminar für Sozialwissenschaften, Harvestehuder Weg 10, Hamburg 13, Germany.

UNIVERSITÄT KIEL, Anthropologisches Institut, Olshausenstr. 40-60, Kiel, Germany. Phys. anth., demography.

*UNIVERSITÄT MAINZ, Institut für Völkerkunde, Mainz, Germany.

UNIVERSITÄT TÜBINGEN, Völkerkundliches Institut, Schloss Hohentübingen, Tübingen, Germany. Ethnol. of E. Polynesia, New Guinea, Mid. Amer.

UNIVERSITÄT ZU KÖLN, Institut für Ur- und Frühgeschichte, Weyertal 125, Köln, Germany. Prehist. arch., paleolithic & neolithic urbaniz., chron. (C14-Pollen-analysis), phys. anth.

UNIVERSITETET I BERGEN, Historisk Museum, Friele gate 3, Bergen, Norway. Arch., ethnol., soc. anth.

UNIVERSITY COLLEGE, Dept. of Anthropology, Gower St., London, W.C. 1, England. Soc. anth., Afr. ethnogr., phys. anth. (human genetics & physiol.).

UNIVERSITY COLLEGE OF RHODESIA AND NYASALAND, Dept. of African Studies, P.B. 167 H. Salisbury, Southern Rhodesia. Gen., Central & E. Afr. sociol., urban studies.

*UNIVERSITY KARLOVY V PRAZE, Anthropologický Ústav, Praha 11, Viničná ul. č. 7, Czechoslovakia.

UNIVERSITY OF BUFFALO, Dept. of Anthropology & Linguistics, Buffalo 14, N.Y., U.S.A. Gen. ling., ling. theory, anth. theory, soc. anth., arch. (Iroquoian).

UNIVERSITY OF CALIFORNIA, Dept. of Linguistics, 2323 Dwinelle Hall, Berkeley 4, Calif., U.S.A.

UNIVERSITY OF CHICAGO, Dept. of Anthropology, 1126 E. 59th St., Chicago 37, Ill., U.S.A. Arch., ethnol., ling., phys. anth., soc. anth.

UNIVERSITY OF DENVER, Dept. of Anthropology, University Park, Denver 10, Colo., U.S.A. Textile technol., plains & S.W. arch.

UNIVERSITY OF HELSINKI, Institute of Sociology, Hallituskatu 13, Helsinki, Finland.

UNIVERSITY OF LONDON, Dept. of Cultural Anthropology, School of Oriental & African Studies, London W.C. 1, England. Asian & E. Afr. anth.

*UNIVERSITY OF LONDON, Institute of Archaeology, 31-34 Gordon Square, London W.C. 1, England.

UNIVERSITY OF OSLO, Anatomical Institute, Karl Johan St. 47, Oslo, Norway. Lapp phys. anth.

UNIVERSITY OF TOKYO, Dept. of Cultural Anthropology, Bunka Jinruigaku Kenkyushitsu, Akamon-iru, Motofuji-cho, Bunkyo-ku, Tokyo, Japan. Ethnol. & arch. of Latin Amer. esp. of the Andes, Japanese soc.

V

*VANDERBILT UNIVERSITY, Dept. of Sociology, Nashville 5, Tenn., U.S.A.

W

WARSAW UNIVERSITY, Dept. of Anthropology, 30 Krak. Przedm. St., Warszawa, Poland. Phys. anth., anthropogenesis, racial classification.

WELSH FOLK MUSEUM, St. Fagans Castle, Cardiff, Wales. Welsh folk life.

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH, 14 E. 71st St., New York 21, N.Y., U.S.A. All fields of anth.

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH, European Headquarters, Burg Wartenstein, bei Gloggnitz, N. Oe, Austria. All fields of anth.

WISCONSIN ARCHEOLOGICAL SOCIETY, 1276 N. 63rd Court, Wauwatosa 13, Wis., U.S.A. Arch.

*Fields of specialization are missing because no data card has been received.

